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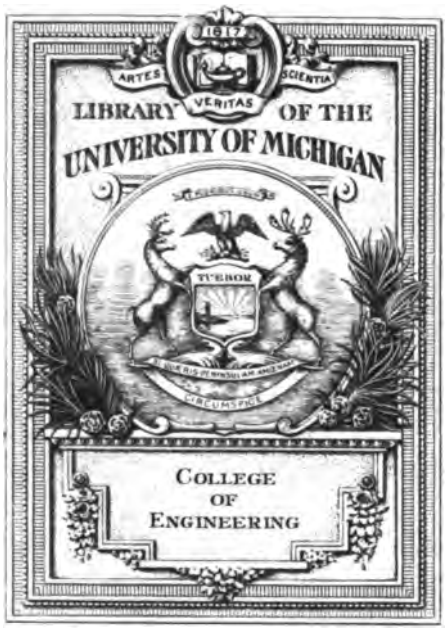

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CONTENTS.

PAGE.

List of Officers v

List of Honorary Members, Honorary Associates, Members, and Associates vii

Objects of the Institution xiii

Bye-Laws and Regulations xiii

Introductory Proceedings xix

PAPERS AND DISCUSSIONS.

A Practical Measurement of the Comparative Fighting Efficiency of Ships of War. By Captain G. H. Noel, R.N., Associate Member of Council 1

Discussion on the above Paper 19

The Use of Water Chambers for reducing the Rolling of Ships at Sea. By P. Watts, Esq., Member of Council 30

Discussion on the above Paper 41

A Mechanical Method of Measuring a Vessel's Stability. By J. H. Heck, Esq., Member 50

Discussion on the above Paper 66

The Stowage of Steam Ships. By F. P. Purvis, Esq., Member 74

Method of arranging the Coal Bunkers of a Steamer, so as to reduce the Ballast to a Minimum. By John Nicholson, Esq. 98

Discussion on the two preceding Papers 105

On Yacht Measurement and Time Allowance for Racing. By Captain J. C. Tuxen 111

Discussion on the above Paper 124

Most suitable Propeller for Shallow Draught. By J. I. Thornycroft, Esq., Member of Council . . 129

Discussion on the above Paper 132

Some further Experience with Triple Compound Engines. By A. E. Seaton, Esq., Member 141

Discussion on the above Paper 146

The Theoretical Duty of Heat in the Steam Engine. By J. Mac Farlane Gray, Esq., Member of Council 154

Discussion on the above Paper 162

On the Application of moderate forced Draught to the Furnaces of small Steam Vessels, upon Mr. P. W. Willan's System. By M. H. Robinson, Esq., Associate 167

Discussion on the above Paper 175

On the Strength of Mild Steel Plates and Rivets of various kinds used in Shipbuilding. By J. G. Wildish, Esq., Member 179

Some Notes on the Strength of riveted Joints. By J. T. Milton, Esq., Assistant Chief Engineer Surveyor to Lloyd's Register of British and Foreign Shipping, Member 204

Discussion on the two preceding Papers 211

Digitized by Google

PAGE.

On the Manufacture of large Forgings for Stern Frames. By D. PURVES, Esq. 227
Discussion on the above Paper 231

On the Efficiency of Marine Boilers. By J. T. MILTON, Esq., Member 240
Discussion on the above Paper 247

Experience in the Use of thick Steel Boiler Plates. By WILLIAM PARKER, Esq., Chief Engineer Surveyor to Lloyd's Register of British and Foreign Shipping, Member of Council 253
Discussion on the above Paper 259

Propelling Machinery for high-speed Ships. By E. A. LINNINGTON, Esq., Member 278
Discussion on the above Paper 285

On the Joy Gear in its bearing on the Question of saving Space in the Engine Rooms of War and other Ships, and its special Adaptability to Triple Expansion Engines. By DAVID JOY, Esq., Member . . 289
Discussion on the above Paper 295

A Diagram of Stability. By Captain PABLO SEOANE, Royal Spanish Navy, Member 298

Concluding Proceedings 302

Memoir of the late J. D'A. SAMUDA, Esq. 307

Nominal Index 311

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Lewis, William.

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Jacobsen, Waldemar.
Jones, Sir Lewis T., G.C.B., Admiral.

Kemp, Dixon (Assoc. Mem. Council).
Key, Sir Cooper, G.C.B., F.R.S., F.R.G.S., Admiral (Vice-President).
King, William Poole, F.R.S.
Kirby, Joseph M.
Kirkaldy, John.
Knudson, Gunnar.
Korshicoff, Nicholas, General, Russian Imperial Navy.
Koznakoff, Nicholas, Capt., Russian Imperial Navy.

Labrano, F., Captain, Italian Royal Navy.
Laws, George A.
Lagane, Amable.
Lautrup, Christian.
Lee, John D.
Liardet, John Evelyn.
Liggins, Henry.
Linnard, Joseph Hamilton.
Lister, W. N., Lieut., R.N.R.
Littleboy, C. W.
Ljungstedt, A. G. A., Capt., Swedish Royal Navy.
Long, A. E.
Long, Samuel, Captain, R.N.
Lüning, J. G.

MacIver, David, M.P.
Mackenzie, James.
Mackenzie, Landseer.

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Martin, Sir Wm. F., Bart., K.C.B., Admiral.
McGregor, J.
McHardy, Wallace Bruce, Commander, R.N.
Medhurst, John T.
Milburn, J. D.
Milne, Sir Alexander, Bart., G.C.B., Admiral of the Fleet (Vice President).
Milward, Thomas.
Moore, William W.
Moreaux, C. F. da S.
Moy, Thomas.

Neal, William George.
Neilson, Walter M., Colonel.
Newman, A. R.
Nicolson, Sir Frederick W. E., Bart., C.B., Admiral (Vice-President).
Noel, G. H., Captain, R.N. (Assoc. Mem. Council).
Northbrook, The Right Hon. the Earl, G.C.S.I. (Vice-President).
Norton, Harold Percival.

Orlando, Paolo.

Pearse, Mountjoy.
Perkins, James.
Petke, Fritz.
Poretchkin, Theodore, Lieut., I.R.N.
Porter, Nicholas Ellis.
Price, Arthur, R.N.

Ramsden, Sir James, Kt. (Assoc. Mem. Council).
Ravensworth, The Right Hon. the Earl of (President).
Rawson, Robert, Hon. Mem. Manchester Literary and Philosophical Society.
Renwick, George.
Rice, J. M., United States Navy.
Riley, James.
Robinson, M. H.
Robinson, Sir R. Spencer, K.C.B., F.R.S., Admiral (Vice-President).
Rothe, H. P., Commodore, Danish Royal Navy.
Rowan, David.
Rowley, Charles J., Captain, R.N.
Rundell, W. W. (Assoc. Mem. Council).
Rundle, Cubitt S.
Rothery, H. C., Q.C.

Samuelson, Sir Bernhard, Bart., M.P.
Sandham, Henry.
Sosoa, Sachin.
Satrustegui, Don Patricia de.
Sandstrom, Theodore C. A., Lieut., Royal Swedish Navy.
Saunders, James Ebenezer.
Schmeman, Alexander.
Schock, J. L.
Schüler, F.
Schunk, Theodore.
Scott, George J.
Serena, Arthur.
Shadwell, Sir Charles F.A., K.C.B., Admiral.
Sinnette, George Mathias.
Small, Thomas.
Smith, Charles William.
Smith, G. Fereday.
Smith, John.
Smith, Josiah T.
Smith, The Right Hon. W.H., M.P. (Vice-President).
Smyth-Pigott, J. H.
Stewart, Sir William Houston, K.C.B., Admiral (Vice-President).
Stothert, G. K.
Stuart, Milton.
Stumore, Frederick.
Sudeley, The Right Hon. Lord.
Suffield, Thomas.
Swinburne, Thomas A., Captain, R.N.
Symonds, Thomas E., Captain, R.N.

Tatham, Hollings.
Taylor, J. M. H.
Temperley, Joseph.
Testa, Charles, Captain, Royal Portuguese Navy.
Thayer, Ignatius E.
Thomson, Andrew.
Thurston, Robert H.
Tindall, William H. (Vice-President).
Tough, R. T.
Tryon, George, C.B., Rear-Admiral, R.N.

Vaz, J. M. P. da F., Captain.

Waddilove, Charles L. D., Vice-Admiral, R.N.
Watson, George.
Watson, Henry James.
Watson, William.
Webb, J. Beavor.
Westhorp, Theophilus.
Wheatley, William Coe.
Wheeler, Joseph, jun.
Whyham, Maurice.
Williams, Samuel.
Williamson, R. H.
Wingfield, G. J., Lieut., R.N.
Wise, William Lloyd.
Wood, George.
Woodall, J. W.
Woodward, Joseph J.
Woolfe, J. S.

Yee, Fung.
Yeo, John, R.N.

Zeysing, Theodore, Prussian Royal Navy.
Ziffer, J. H.

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OBJECTS OF THE INSTITUTION.

The objects of the Institution of Naval Architects—which was established to promote the Improvement of Ships, and of all that specially appertains to them—are comprised under three heads :—

First, the bringing together of those results of experience which so many shipbuilders, marine engineers, naval officers, yachtsmen, and others acquire, independently of each other, in various parts of the country, and which, though almost valueless when unconnected, doubtless tend much to improve our Navies when brought together in the printed Transactions of an Institution.

Secondly, the carrying out, by the collective agency of the Institution, of such experimental and other inquiries as may be deemed essential to the promotion of the science and art of shipbuilding, but are of too great magnitude for private persons to undertake individually.

Thirdly, the examination of new inventions, and the investigation of those professional questions which often arise, and were left undecided before the establishment of this Institution, because no public body to which professional reference could be made then existed.

BYE-LAWS AND REGULATIONS.

CONSTITUTION.

1. The Institution of Naval Architects shall consist of four classes, *viz.*, Members, Associates, Honorary Members, and Honorary Associates.

2. *Members.*—The Class of Members shall consist exclusively of Naval Architects, and Marine Engineers conversant with Naval Architecture.

3. *Associates.*—The Class of Associates shall consist of persons who are qualified, either by profession or occupation, or by scientific or other attainments, to discuss with Naval Architects the qualities of a ship, or the construction, manufacture, or arrangement of some part or parts of a ship or her equipment.

4. *Honorary Members.*—The Class of Honorary Members shall consist of persons who are eligible as Members, and upon whom the Council may see fit to confer an honorary distinction.

5. *Honorary Associates.*—The Class of Honorary Associates shall consist of persons who have contributed to the improvement of ships or their equipment, and upon whom the Council may see fit to confer an honorary distinction.

ELECTION AND DUTIES OF OFFICERS.

6. The Officers of the Institution shall consist of a President, Vice-Presidents, Members of Council, Associate Members of Council (not exceeding in number one-third the number of Members of Council), a Treasurer, two Auditors of Accounts, and a Secretary or Secretaries.

7. A General Meeting of the Members and Associates of the Institution shall be held annually before Easter in each year; and at this Annual General Meeting the Members of Council, Associate Members of Council, Treasurer, and Auditors for the ensuing year shall be elected.

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8. At the annual General Meeting Members only shall vote in the Election of Members of Council, and both Associates and Members in the election of Associate Members of Council, the Treasurer and the Auditors.

9. As soon as may be convenient after the Annual General Meeting, the newly-erected Members and Associate Members of Council shall meet and elect the President and Vice-Presidents for the year, Members and Associate Members of Council both voting.

10. *President.*—Both Members and Associates of the Institution shall be eligible for election as President. The President shall preside over all meetings of the Institution, and of Officers of the Institution, at which he is present, and shall regulate and keep order in the proceedings.

11. *Vice-Presidents.*—Both Members and Associates of the Institution shall be elegible for election as Vice-Presidents. In the absence of the President, one of the Vice-Presidents shall preside at the General Meetings of the Institution, and shall regulate and keep order in the proceedings.

12. In case of the absence of the President and of all the Vice-Presidents, the Meeting may elect any Member of Council or Associate Member of Council, and in case of their absence any Member present to preside.

13. The Chairman at any Meeting of the Council of the Institution, when the votes of the Meeting, including his own, are equally divided, shall be entitled to give a casting vote.

14. Persons holding the office of Vice-President shall at all times be entitled to sit and vote with the Council.

15. *Past Presidents and Vice-Presidents.*—All Members who have held the posts of President and Vice-President shall, while their connection with the Institution as Members lasts, be entitled to sit and vote with the Members of Council.

16. *Members of Council.*—Members only shall be eligible for Election as Members of Council at the Annual General Meeting.

17. *Associate Members of Council.*—Associates only shall be eligible for Election as Associate Members of Council at the Annual General Meeting.

18. The Direction and Management of the Institution shall be vested in the Council for the time being, the Associate Members voting with the Members of Council in all cases, except in the decision of questions directly affecting the forms of ships and the construction of their hulls.

19. The Council shall meet as often as the business of the Institution requires, and at every Meeting five Members of the Council shall form a *quorum.*

20. The Council may appoint Committees to report to them upon special subjects.

21. All questions shall be decided in the Council by vote; but at the desire expressed in writing, of any four Members or Associate Members present, the determination of any subject shall be postponed to the succeeding Meeting of the Council.

22. An annual statement of the funds of the Institution, and of the receipts and payments of the past year, shall be made under the direction of the Council, and after having been verified and signed by the Auditors, shall be laid before the Annual General Meeting.

23. The Council shall draw up an Annual Report on the state of the Institution, which shall be read at the Annual General Meeting.

24. It shall be the duty of the Council to adopt every possible means of advancing the Institution, to provide for properly conducting its business in all cases of emergency, such as the death or resignation of Officers, and to arrange for the publication of the Papers read at the Meetings, or of such documents as may be calculated to advance the objects of the Institution.

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25. *Treasurer.*—Only Bankers, or Members of Council, or persons who have been Members of Council and are still Members of the Institution, shall be eligible for election as Treasurer.

26. *Auditors.*—All Members and Associates of the Institution shall be eligible for election as Auditors.

27. The Auditors shall have access at all reasonable times to the Accounts of the pecuniary transactions of the Institution; and they shall examine and sign the annual statement of the Accounts before it is submitted by the Council to the Annual General Meeting.

28. *Secretary.*—The Secretary or Secretaries shall be elected by the Council, and shall be removable at the will of the Council, after due notice given. The salary of the Secretary or Secretaries shall be fixed by the Council.

29. It shall be the duty of the Secretary, under the direction of the Council, to conduct the correspondence of the Institution; to attend all Meetings of the Institution and of the Council; to take Minutes of the proceedings of such Meetings; to read the Minutes of the preceding Meeting; to announce donations made to the Institution; to superintend the publication of such Papers as the Council may direct; to have charge of the library, museum, and offices of the Institution; and to direct the collection of subscriptions and the preparation of accounts. He shall also engage, and be responsible for, all persons employed under him, and generally conduct the ordinary business of the Institution.

30. In January of each year the Council shall meet and prepare Lists for the election of the Council for the ensuing year. These Lists shall be as follows, namely :—

1*st.* A List of the names of the President, Vice-Presidents, and Treasurer for the ensuing year to be submitted at the Annual General Meeting, for their election in a body.

2*nd.* Lists for the election of the Ordinary Members and Associate Members of Council. This List shall contain the names of the existing Ordinary Members of Council and Associate Members of Council, and at least *six* new names of Members, and at least *two* of Associates of the Institution.

31. No addition shall be made to the total number of Ordinary Members of the Council until, by death or resignation, their numbers shall have been reduced below twenty-four, after which their numbers shall be raised to and preserved at twenty-four. And no addition shall be made to the Associate Members of Council until, by death or resignation, their numbers shall have been reduced below eight, after which their numbers shall be raised to and preserved at eight, always exclusive of the President, Vice-Presidents and Treasurer.

32. At the date of issuing the Syllabus of the Annual General Meetings in each year, the complete Lists proposed by the Council for the Ordinary Council for the ensuing year shall be printed, and sent to all Members to serve as Balloting Lists. These Lists shall contain, first, the names of the existing ordinary Members of Council at the time of the preparation of the Balloting List, together with six new names of Members of the Institution (or, if the number shall have been reduced below twenty-four, then so many new names as shall be needed to bring the number up to thirty), and the whole of these names shall be printed in alphabetical order. Secondly, the names of the existing Associate Members of Council at the time of the preparation of the Balloting List, together with two new names of Associates of the Institution (or, if the number shall have been reduced below eight, then so many new names as shall be needed to bring the number up to ten), and these names also shall be printed in alphabetical order. From these Lists the new Council shall be elected. Every Member shall be at liberty to strike out any name on the Lists, but he must strike out *six* names from the List of Members of Council, and *two* names from the List of Associate Members of Council.

33. A similar Balloting List (in which, however, the names of the ordinary Members of Council proposed for

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Election shall not be included) shall be printed and sent to all Associates of the Institution, to serve as a Balloting List for Associates, from which the voting for Associate Members of Council shall be taken. Every Associate shall be at liberty to strike out any names on that List; but he *must* reduce the number by striking out *two* names from the List of Associate Members of Council.

34. The Balloting Lists may be sent by post or otherwise to the Secretary, so as to reach him before the day and hour named for the Annual General Meeting, or they may be personally presented by the Members and Associates at the opening of the Annual General Meeting.

35. At the opening of the Annual General Meeting the order of business shall be:—

(1.) To read and consider the Reports of the Council and Treasurer.

(2.) To read the List of Officers and Nomination for Council for the ensuing year, proposed by the Council.

(3.) The Chairman shall next put to the Meeting the List containing the names of the President, Vice-Presidents and Treasurer for election for the ensuing year.

(4.) The Chairman shall next nominate two Scrutineers (of whom one only shall be a Member of the existing or proposed Council), and shall hand to them the Ballot Boxes containing the Voting Papers for the ordinary Members of Council and Associate Members of Council; and

(5.) The Scrutineers, assisted by the Secretary and his clerks, shall receive the Ballot Papers which may have reached him, and all others which may be presented by Members or Associates at the Meeting. The Scrutineers shall then retire, and verify the Lists, and count the votes; and shall, not later than the following day, report to the Chairman the names which have obtained the greatest number of votes, subject to the conditions of the Ballot. The Chairman shall then read the List presented by the Scrutineers, and shall declare the gentlemen named in the List to be duly elected.

(6.) After the Ballot shall have been taken, and the Scrutineers have retired, the Meeting will proceed to the other business before it.

36. The new Council and Officers shall take office immediately after the close of the Annual General Meeting.

DESIGNATION OF MEMBERS AND ASSOCIATES.

37. Any Member, Associate, Honorary Member, or Honorary Associate, having occasion to designate himself as belonging to the Institution, shall state the class to which he belongs according to the following abbreviated forms, *viz.*, M.I.N.A.; Assoc. I.N.A.; Hon. Mem. I.N.A.; Hon. Assoc. I.N.A.

ELECTION OF MEMBERS AND ASSOCIATES.

38. *Admission of Members.*—Every Candidate for admission into the Class of Members, or for transfer into that Class from the Class of Associates, shall be more than twenty-five years of age, and shall comply with the following regulation:—

He shall submit to the Council a statement showing that he has been professionally engaged in shipbuilding or marine engineering for at least seven years in some public or private shipbuilding establishment, or marine engine works, and setting forth the grounds upon which he bases his claims to be considered a professional Naval Architect or Marine Engineer conversant with Naval Architecture, and to be admitted as such to the Membership of the Institution. This shall be signed by at least three Members whose signatures shall certify their personal knowledge of the Candidate, and approval of his statement;

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or, in the case of persons not British born, the signatures of three Members shall be required, in confirmation of their personal knowledge of the Candidate's scientific reputation.

39. These preliminary conditions being satisfied, the Council shall then consider whether the practical experience and professional attainments of the Candidate are such as entitle him to be brought forward by the Council as a Naval Architect, or Marine Engineer conversant with Naval Architecture. If four-fifths at least of the received votes of the professional Members of the Council are in favour of his application, and such four-fifths constitute a majority of the professional Members of the Council, his proposal for admission shall be submitted to the Members of the Institution (who shall have access to the applicant's statement), at an Ordinary Meeting of the Institution, for them to vote upon, the voting to be by ballot, should a ballot be demanded.

40. *Admission of Associates.*—Candidates for Associateship shall submit to the Council a proposal for their admission, setting forth therein a statement of their claims to be admitted as Associates. Their proposal, if approved by the Council, shall be submitted by them at an Ordinary Meeting of the Institution, for the Members and Associates jointly to vote upon, the voting to be by ballot, should a ballot be demanded.

41. The proportion of votes for deciding the election of Members and Associates shall be at least four-fifths of the numbers recorded.

SUBSCRIPTIONS.

42. Each Member and Associate shall pay an Entrance Fee of two guineas, and an Annual Subscription of two guineas in advance; the first Subscription being payable on his election, and all future ones on the 1st day of January of each year. Any Member or Associate withdrawing from the Institution after that date is still liable for the amount of Subscription due on that day.

43. Any Member or Associate may compound for his Annual Subscription, for life, by a single payment of not less than thirty guineas.

44. No person's name shall be entered on the Roll as Member or Associate of the Institution nor possess the privileges of Membership (except it be on the honorary list) until he shall have paid his first subscription or the life composition, and if the payment be delayed for more than twelve months from the date of his election, the same shall be void unless the Council otherwise direct.

45. The Secretary shall at the close of every year notify to all Members and Associates whose subscription for that year shall not have been paid, that it will be his duty to report accordingly to the Council, and he shall at the same time furnish the person whose subscription is in arrear with copies of this and of the two following Rules.

46. The Secretary shall before Easter in every year lay before the Council a list of all Members and Associates whose subscriptions for the two previous years shall be still unpaid, and unless the Council shall otherwise direct, the names of those in arrear shall be expunged from the Roll of Members and Associates, and shall not be replaced without re-election in due form. Provided always that the Council shall at any time within two years therefrom have power to dispense with such re-election, and to restore the name to the Roll upon payment of all subscriptions then due, and upon cause being shown to the satisfaction of the Council why such subscriptions were not previously paid.

47. Nothing herein contained shall prejudice the right of the Institution to the legal recovering of all arrears of subscriptions up to the date of striking the name off the Roll.

48. In case the Council should be of opinion that any Member, who has been long distinguished in his professional career, from ill-health, advanced age, or other sufficient causes, should not be called upon to continue

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his annual subscription, they may remit it. Also they may remit any arrears which are due from an individual, or may accept a collection of books, or drawings, or models, or other such contribution as, in their opinion, under the circumstances of the case, may entitle the person to be enrolled as a Life Subscriber, or to enable him to resume his former rank in the Institution which may have been in abeyance from any particular causes. These cases must be considered and reported upon by a Sub-Committee named for the purpose.

49. In case the expulsion of any individual shall be judged expedient by ten or more Members, and they think fit to draw up and sign a proposal requiring such expulsion, the same being delivered to the Secretary shall be by him laid before the Council for consideration. If the Council, after due inquiry, do not find reason to concur in the proposal, no entry thereof shall be made in any Minutes, nor shall any public discussion thereon be permitted; but if the Council do find good reason for the proposed expulsion they shall direct the Secretary to address a letter to the person proposed to be expelled, advising him to withdraw from the Institution. If that advice be followed, no entry on the Minutes nor any public discussion on the subject shall be permitted; but if that advice be not followed, nor a satisfactory explanation given, the Council shall call a Special General Meeting of Members and Associates, for the purpose of deciding on the question of expulsion; and if two-thirds of the persons present at such Special General Meeting, providing the number so present be not less than thirty, vote that such individual be expelled, the Chairman of that Meeting shall declare such expulsion accordingly, and the Secretary shall communicate the same to the individual.

MEETINGS.

50. Meetings for the Reading of Papers shall be held as frequently, and at such times, as the Council may determine.

TRANSACTIONS.

51. The *Transactions* of the Institution, including the Papers read at the Ordinary Meetings, and Reports of the Discussions by which they are followed, shall be edited by the Secretary, and printed under the direction of the Council.

52. A copy of each Volume of *Transactions* shall be sent free to every Member and Associate.

53. The Secretary, under the direction of the Council, may dispose of the surplus stock of *Transactions* which have been published more than three years, at a price of not less than One Guinea a volume, provided a sufficient number remain on hand to supply the probable demand of new Members and Associates to complete their sets by the purchase of the back Volumes.

CHANGE OF ADDRESS.

54. Members and Associates are particularly requested to communicate to the Secretary any change of address.

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INTRODUCTORY PROCEEDINGS.

THE Meetings of this, the Twenty-sixth Session of the Institution of Naval Architects, were held on the 25th, 26th and 27th March, 1885, in the Hall of the Society of Arts, John Street, Adelphi.

The opening Meeting was presided over by the Right Hon. Earl of RAVENSWORTH, President of the Institution, who commenced the proceedings by calling upon the Secretary, Mr. GEORGE HOLMES, to read the Report of the Council, which was as follows:—

REPORT OF COUNCIL, 1885.

"The Council reports, with much satisfaction, that the financial condition of the Institution continues to be satisfactory. In spite of an increased expenditure, chiefly under the head of Printing, of £123 14*s.* 9*d.*, the balance in hand at the end of last year was £199 11*s.* 1*d.* in excess of that of the year preceding, while during the same period the Library Fund was increased by the sum of £106 18*s.* 8*d.*—a result chiefly due to the large number of New Members and Associates who joined the Institution in the year 1884. The satisfactory amount realised under the head Sales of Volume, *viz.*, £170 15*s.* 7*d.*, shows that the value of the Publications of the Institution is steadily growing in the appreciation of the outside public, the larger portion of this item having been obtained from non-members.

"In consequence of the heavy increase in the item of Printing and Engraving, the Council has made new arrangements with regard to the publication of the *Transactions*, which will, it is hoped, result in a very considerable saving. In future, the *Transactions* will be published in about two months after the Annual Meetings, instead of four months, as has latterly been the practice, or seven months, as was formerly the case. This comparatively rapid publication will render unnecessary the issue of an edition of the Papers, as read at the Meetings, to all Members and Associates. This issue constituted, in fact, a first edition of the *Transactions*, and involved the Institution in an expense of about One hundred pounds, which will in future be saved.

"In order to expedite the production of the *Transactions* New Rules have been framed for the correction of Reports of Discussions, copies of which will be forwarded to each speaker, together with the report of his remarks. Members are earnestly invited to co-operate with the Council in their endeavours to secure an early publication of the *Transactions* by a strict compliance with the New Rules.

"The Rules for the admission of Members and Associates have been under the consideration of a Committee of the Council specially appointed. The Old Rules were found to be somewhat difficult in application, and have never been put fully into operation. The Committee, whose Report is appended, has made sundry suggestions for the alteration of the Rules, which will be submitted for the approval of the General Meeting."

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REPORT OF THE COMMITTEE APPOINTED TO CONSIDER THE METHOD OF ELECTING MEMBERS AND ASSOCIATES.

" The Committee appointed on the 4th of April last to consider the method of electing Members and Associates met " this day (Mr. Samuda being absent through illness), and agreed to the following Report :—

" The Committee recommend the following alterations in the Rules :

" (*a*) In the second Clause of Rule 38, after the words 'Marine Engineer,' to introduce the words 'conversant " ' with Naval Architecture' " (*see* Rule 2).

" (*b*) At the end of Rule 39 and of Rule 40 to omit the words 'by ballot' and to substitute the words 'the voting " ' to be by ballot, should a ballot be demanded.'

" The Committee are of opinion that advantage would result from greater strictness being exercised in respect of the " 'Statements' of claim submitted by applicants for Membership, which statements should, in the Committee's opinion, " always comprise words to the effect that those Members who sign the statement are satisfied that the Candidate is (as the " case may be) either a competent Naval Architect or a Marine Engineer, qualified by his conversance with Naval " Architecture to become a Member.

" The Committee have suggested the adoption of a simplified 'Form' of application for the admission of Associates.

" E. J. REED,
" *Chairman.*

" *Dated* 11*th December*, 1884."

[BALANCE SHEET.

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Institution of Naval Architects.

Statement of Income and Expenditure for the Year 1884.

Dr.

		£ s. d.	£ s. d.
1883.			
Dec. 30th.	To Balance :—		
	In Treasurer's hands	532 5 8	
	In Secretary's hands	63 2 3	
			595 7 11
1884.			
Dec. 31st.	Annual Subscriptions		1,140 9 2
	Sales of Volumes		170 15 7
	Admiralty Grant		250 0 0
			£2,156 12 8

Cr.

		£ s. d.	£ s. d.
1884.			
Dec. 31st.	By Cash paid :—		
	Rent of Rooms		200 0 0
	Cleaning Rooms		37 14 0
	Attendance at Meetings		15 2 6
	Reporting at ditto		45 0 0
	Advertising		27 7 4
	Printing Vol. XXIV.		
	Engraving Vol. XXIV. ...	502 3 4	
	Binding Vol. XXIV.		
	Miscellaneous Printing ...		
	Photograph Vol. XXIV. ...	12 0 0	
	Despatch of Volume	33 3 11	
			547 7 3
	Stationery		10 2 4
	Insurance		2 10 0
	Subscription Returned		2 2 0
	Bankers' Charges		14 2
	Petty Disbursements		47 2 5
	Furniture and Repairs		2 15 5
	Secretary's Salary	337 10 0	
	Clerk's and Messenger's ditto ...	80 0 0	
	Extra Attendance	6 6 3	
			423 16 3
	Balance :—		
	In Treasurer's hands	754 12 2	
	In Secretary's hands		
	Annual Subscriptions	8 16 5	
	Current Expenses	31 10 5	
			794 19 0
			£2,156 12 8

Library Account for the Year 1884.

		£ s. d.	£ s. d.
1883.			
Dec. 30th.	To Balance :—		
	In Treasurer's hands	302 8 9	
	In Secretary's hands	2 2 0	
			304 10 9
1884.			
Dec. 31st.	Entrance Fees		115 10 0
			£420 0 9

		£ s. d.
1884.		
Dec. 31st.	By Library Expenses	8 11 4
	Balance :—	
	In Treasurer's hands	411 9 5
		£420 0 9

J. D'A. SAMUDA, *Treasurer*,
GEORGE HOLMES, *Secretary*.

We have examined the above written entries with the books and vouchers, and find them correct.

JAMES MACKENZIE, }
JOHN CORRY, } *Auditors*.

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The following is a List of Donations to the Library for the year 1884:—

"Proceedings of the Royal Society." *Presented by the Royal Society.*

"Minutes of the Proceedings of the Institution of Civil Engineers," Vols. LXXV., LXXVI., LXXVII., and LXXVIII. Name Index, Vol. I to LVIII. *Presented by the Institution of Civil Engineers.*

"Proceedings of the Institution of Mechanical Engineers," for 1884. *Presented by the Institution of Mechanical Engineers.*

"Journal of the Iron and Steel Institute," for 1884. *Presented by the Iron and Steel Institute.*

"Journal of the Society of Arts," for 1884. *Presented by the Society of Arts.*

"Transactions of the Institution of Engineers and Shipbuilders in Scotland," Vol. XXVII. *Presented by the Institution of Engineers and Shipbuilders in Scotland.*

"Transactions of the Society of Engineers," for 1884. *Presented by the Society of Engineers.*

"Annual Report of the Board of Regents of the Smithsonian Institution," for 1882. *Presented by the Board of Regents.*

"Lloyd's Register of British and Foreign Shipping," for 1884-85. *Presented by the Committee of Lloyd's Register.*

"Lloyd's Register of Yachts," for 1884. *Presented by the Committee of Lloyd's Register.*

"Annals of Lloyd's Register of British and Foreign Shipping." *Presented by the Chairman and Committee of Lloyd's Register of British and Foreign Shipping.*

"Underwriters' List of Iron Vessels," for 1884-85. *Presented by the Underwriters' Registry of Iron Vessels, Liverpool.*

"The Scientific Proceedings of the Royal Society of Dublin," Vol 4. *Presented by the Royal Society of Dublin.*

"North of England Institute of Mining Engineers," Vol. XXXII. *Presented by the North of England Institute.*

"Engineer," for 1884. *Presented by the Proprietors.*

"Engineering," for 1884. *Presented by the Proprietors.*

"Iron," for 1884. *Presented by the Proprietors.*

"Field," for 1884. *Presented by the Proprietors.*

"Army and Navy Gazette," for 1884. *Presented by the Proprietors.*

"Shipping Gazette," for 1884. *Presented by the Proprietors.*

"Steamship," for 1884. *Presented by the Proprietors.*

"Saturday Review," for 1884. *Presented by the Proprietors.*

"Marine Engineer," for 1884. *Presented by the Proprietors.*

"English Mechanic," for 1884. *Presented by the Proprietors.*

"Revue Maritime," for 1884. *Presented by the French Ministry of Marine.*

"Rivista Maritima," for 1884. *Presented by H. Clauson, Esq.*

"Proceedings of the Royal Society of Victoria," Vol. XIX. *Presented by the Royal Society of Victoria.*

"Germanischer Lloyd." *Presented by* ALFRED A. DIETRICH, Esq.

"Annalen Fur Gewerbe and Bauwesen." *Presented by the Proprietor.*

"Wochenschrift des Osterreichischen Ingenieur-und Architekten-Vereines. *Presented by the Society.*

"Science," for 1884. *Presented by the Proprietors.*

"The American Engineer," for 1884. *Presented by the Proprietors.*

"The Journal of the Franklin Institute," for 1884. *Presented by the Institute.*

"Proceedings of the United States Naval Institute," for 1884. *Presented by the Institute.*

"Ordnance Notes," for 1884. *Presented from the Ordnance Office, War Department, Washington, D.C., United States.*

"Applied Mechanics." By Professor J. H. COTTERILL, F.R.S. *Presented by the Author.*

"The Wave of Translation in the Oceans of Water, Air and Ether." By J. SCOTT RUSSELL, Esq., F.R.S. *Presented by Mrs. Scott Russell.*

"Collection de Plans ou Dessins de Navires et de Bateaux Anciens ou Modernes." *Presented by Vice-Admiral E. Paris.*

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The following Gentlemen (having been duly recommended by the Council) were unanimously elected:—Mr. Henry Horatio Ash, Foreman H.M. Dockyard, Devonport; Mr. Sydney Walker Barnaby, Naval Architect, Messrs. J. I. Thornycroft & Co., Church Wharf, Chiswick, London; Mr. John Black, Assistant Constructor, H.M. Dockyard, Devonport; Mr. Arthur William Blake, Shipbuilder, Great Yarmouth; Mr. James Stewart Clark, Shipbuilding Manager, Barrow Shipbuilding Company, Barrow-in-Furness; Monsieur Victor Daymard, Chief Engineer Cie. Gle. Transatlantique, Paris; Mr. Frederick Edwards, Consulting Marine Engineer, Bishopsgate Street, London; Mr. Neville Evans, Superintendent Engineer, Messrs. Frederick Leyland & Co., Liverpool; Mr. Edward Faron, Engineer and Constructor, Messrs. John Roach & Son, Shipyard, Chester, New York, U.S.A.; Mr. John Henry Heck, Engineer Surveyor, Lloyd's Register, Newcastle-on-Tyne; Mr. William Laing, Superintendent Engineer, Messrs. Alexander A. Laird & Co., Shipowners, Glasgow; Mr. James Henry Morgan, Messrs. Morgan & Medhurst, Limehouse, London; Mr. John Mowat, Chief Draughtsman, Messrs. Joseph L. Thompson & Sons, North Sands, Sunderland; Mr. Alexander Thomson Orr, Messrs. Hall, Russell and Co., Aberdeen, N.B.; Mr. Peter Phorson, Shipyard Manager, Messrs. J. L. Thompson and Sons, North Sands, Sunderland; Monsieur Pierre Leon Piaud, Bureau Veritas, Paris; Mr. John Sinclair Pirrie, Managing Director, Carnac Ironworks Company, Bombay, India; Mr. S. Popper, Constructor, Imperial and Royal Austrian Navy, Trieste; Mr. James Readhead, Manager, Shipbuilding Department, Messrs. John Readhead and Co., North Shields; Mr. Norman Selfe, Sydney, New South Wales; Captain Pablo P. Seoane, Naval Architect, Royal Spanish Naval Commission, London; Mr. John Stewart, Engineer and Shipbuilder, Blackwall, London; Mr. Alfred Oscar Trijgger, Master of Swedish Government School of Naval Architecture, Gothenburg, Sweden; Mr. Saul Warner, Surveyor, Board of Trade, London, Mr. Walter Henry Wilson, Partner in the Firm of Messrs. Harland & Wolff, Belfast; Mr. Robert Wyllie, Manager, Messrs. Thomas Richardson & Sons, Hartlepool Engine Works, Hartlepool.

The SECRETARY next read the following list of names nominated by the Council to serve as Ordinary and Associate Members of Council for the ensuing year:*—Mr. Frederick K. Barnes, Mr. William J. Bone, Mr. William Denny, Mr. Raylton-Dixon, Mr. James Dunn, Professor Francis Elgar, Mr. J. MacFarlane Gray, Mr. Henry Green, Mr. Edward J. Harland, Mr. Robert H. Humphrys, Mr. John Inglis, junior, Mr. William John, Mr. Alexander C. Kirk, Mr. James Laing, Mr. Henry H. Laird, Mr. Andrew Leslie, Mr. Frank Marshall, Mr. Benjamin Martell, Mr. J. T. Milton, Mr. Henry Morgan, Mr. William Parker, Mr. William Pearce, Mr. John Price, Mr. George B. Rennie, Mr. Richard Sennett, Mr. J. Rodger Thomson, Mr. John I. Thornycroft, Mr. Phillip Watts, Mr. Bernard Waymouth, Mr. H. H. West, Mr. John White, and Mr. William H. White as Members of Council; and the Most Hon. the Marquis of Ailsa, Sir Frederick J. Bramwell, F.R.S.; Professor J. H. Cotterill, F.R.S.; Mr. E. A. Cowper, Mr. R. E. Froude, Mr. Dixon Kemp, Captain G. H. Noel, R.N.; Sir James Ramsden, Kt.; Mr. H. C. Rothery, Q.C.; and Mr. W. W. Rundell, as Associate Members of Council.

The PRESIDENT (Earl of Ravensworth) next put to the Meeting the following list, containing the names of the President and Vice-Presidents proposed for the ensuing year, and which was unanimously adopted:—The Right Hon. the Earl of Ravensworth, President; H.R.H. Vice-Admiral the Duke of

* For List of Members of Council actually elected, *see* p. xxxii.

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Edinburgh, K.G.; the Right Hon. the Earl of Northbrook, G.C.S.I.; the Right Hon. Hugh C. E. Childers, M.P.; the Right Hon. G. J. Göschen, M.P.; Admiral the Right Hon. Lord John Hay, K.C.B.; Admiral the Right Hon. Sir John Dalyrmple Hay, Bart., M.P., C.B., D.C.L.; the Right Hon. W. H. Smith, M.P.; Admiral the Hon. Arthur Duncombe; Sir W. G. Armstrong, Kt., C.B., D.C.L., F.R.S.; Sir Frederick J. Bramwell, F.R.S.; Sir Thomas Brassey, K.C.B., M.P.; Admiral Sir Alexander Milne, Bart., G.C.B.; Admiral Sir Frederick W. E. Nicolson, Bart., C.B.; Admiral Sir Cooper Key, G.C.B., F.R.S.; Sir E. J. Reed, K.C.B., F.R.S., M.P.; Admiral Sir Robert Spencer Robinson, K.C.B., F.R.S.; Admiral Sir W. Houston Stewart, K.C.B.; Vice-Admiral Thomas Brandreth; Mr. Nathaniel Barnaby, C.B.; Mr. F. K. Barnes; Mr. Thomas Chapman, F.R.S., F.S.A.; Mr. Peter Denny, F.R.S.E.; Mr. George W. Rendel; Mr. J. D'Aguilar Samuda; Mr. W. H. Tindall; Mr. John White; Mr. James Wright, C.B.; and Mr. Joseph Woolley, M.A., LL.D., as Vice-Presidents; Mr. J. D'Aguilar Samuda, Treasurer; and Mr. John Corry and Mr. James Mackenzie as Auditors.

The PRESIDENT then nominated Mr. George B. Rennie and Mr. George Colby Mackrow as Scrutineers to examine the voting papers.

The PRESIDENT: Gentlemen, I beg to move that Rule 39 be altered by introducing the words "conversant with Naval Architecture" after the words "Marine Engineer," and that Rules 39 and 40 be altered by omitting the words "by ballot" and substituting the words, "the voting to be by ballot, should a ballot be demanded."

Mr. J. D'A. SAMUDA: My Lord, I beg to second that motion.

The motion, on being put to the Meeting, was carried unanimously.

The PRESIDENT then proceeded to deliver the following Opening Address:—

Gentlemen, I rise to ask your indulgence for a short time while I make a few observations to you, as is usual on these occasions on the part of the President, in the shape of an Opening Address. I wish to express to you all the very great pleasure that it gives me to meet you again. At the same time, Gentlemen, in making a retrospect of events of the past year, I regret that truth does not allow me to find much matter for congratulation. Trade has been almost everywhere more or less bad during the past year, and, if I might borrow a phrase that I saw used the other day, which is a very significant phrase, in one of the leading journals, "Manufacturing industry in the civilized world has "been (and I am afraid is still) at a very low ebb." And I need hardly tell you, Gentlemen, who are practical men, that the depression which trade has suffered almost everywhere has acted with peculiar severity upon the two great industries of shipowner and shipbuilder. Probably these two industries have suffered as much as, or more, than any other, with perhaps the one exception of the manufactured iron trade, which, of course, is very closely associated with the shipbuilding trade. And one very marked feature of the depression, out of which we have not by any means emerged as yet, was the rapidity with which it came upon the shipbuilding industry. At the same time, it is rather curious to observe, as I think I shall be able to show you, that the depression did not affect quite in an equal degree all our ports. It affected all, but as I said, not in quite an equal degree; because I find that in the Mersey the actual amount of tonnage produced last year was in excess of that produced in 1883. It was 45,000 tons last year, as against 43,000 tons in 1883.

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The Clyde, also—although I am afraid the depression has largely affected the industry there—presents a very handsome output, considering the circumstances of the year; for the Clyde appears to have produced 299,000 tons last year, as against 417,000 tons in 1883, showing a diminution of no less than 118,000 tons in that great Port. The North-Eastern Ports present a total something less than the Clyde, namely, 297,000 tons last year; but I grieve to say the deficiency is represented by no less a figure than 300,000 tons of shipping during the past year, in that group of Ports which is known as the North-Eastern Ports.

Steam vessels, I believe I may say with truth, are building at this moment at 25 per cent. less than they were building at two years ago. No doubt these low prices may attract orders to some extent, and I am happy to hear within the last few days that symptoms of something like greater activity are apparent in the Port of the Tyne, with which I am, closely connected. I do not think that the whole diminution in the production of tonnage during the past year, taking the kingdom over, in all our Ports, can possibly be put at a less figure than 500,000 tons.

Now, Gentlemen, I want you for a moment to consider what that means. I will put the average at £15 per ton, spread over the whole, and I do not think you will say that that that is an excessive average, although we know that many steamers have been sold at prices very far below that—something like £9 per ton, even less; but taking £15 per ton as the average, 500,000 tons diminished production of tonnage means 7½ millions of money withdrawn from the labour markets of our great Ports. That, I think, will go a long way to account for the amount of misery and distress and privation which too many of us who are connected with, and who live in the neighbourhood of these Ports, have witnessed during the past winter on the part of the industrial classes (and there are many trades as you know connected with shipbuilding, apart from the shipbuilding trade itself), and which has defied almost the efforts of our relief committees to cope with it; but which I must say, from personal observation, they have borne with an amount of courage and patience which does the working classes the utmost credit.

There is a feature in shipbuilding in the past year to which I wish particularly to call your attention. Steel is still more than ever taking the place of iron. I find that out of the total of 299,000 tons produced upon the Clyde, no less than one hundred and twenty-eight vessels, with an aggregate of 131,000 tons, were of steel. One eminent firm on the Clyde which we know very well constructed the whole of their vessels of steel. There is another feature in the Clyde trade which I might allude to in passing: I believe I am correct in saying that the average size of the vessels built upon the Clyde was considerably less last year than it had been in previous years; but one great firm which we know also very well built, I think, only six vessels, but they averaged upwards of 5,000 tons each, which is a very remarkable output from one establishment.

On the Tyne, where our shipbuilders were later in adopting steel in the construction of large steamers, it shows a considerable increase; there were no less than seventeen vessels, with an aggregate tonnage of 10,564 tons, built of steel on the Tyne; and at the other ports the aggregate amount of steel tonnage was 22,340 tons; the total amount of steel tonnage produced in the past year appears to have been 164,034 tons.

There is another circumstance which will be interesting to many of our Members, namely, the increasing use of the triple expansion engine, and the increase of high pressures. I find that 150 pounds

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pressure is now a common pressure—170 pounds has been reached, and that I need not say is entirely due to the use of steel in the manufacture of ships' boilers. No less than thirty-nine steamers have been built under Lloyd's Rules, fitted with triple expansion engines, in the last two years. Forty-three sets are now building, and I believe I am correct when I say that the engineers of the Clyde, the Tyne, the Wear, and the Tees, all predict as a certainty that this is to be the marine engine of the future. I would also allude in passing to the economic results of high pressure, and of the use of the three-cylinder engine. It amounts to no less a saving in fuel than from twenty to twenty-five per cent., which is a strong inducement no doubt to adopt these high pressures and this particular form of engine.

Gentlemen, I have alluded to the suddenness with which the crisis came upon the country. I have a few figures here to prove that, and to prove also that the severity of the crisis was felt in a very much greater degree during the latter six months of the year, than the former. Messrs. Lloyd and Lowe's Steam Ship Circular of January 3rd last, shows that taking 500 tons as a basis, the total net increase of steamers *added to Register* in 1884 was 444,516 tons, as compared with 633,868 in 1883, the said increase having been 306,046 tons in the *first* six months of the year, and 143,469 tons only in the last six months. There is one point I should like to mention. I have said that it is possible, and it is probable we will hope, that the low prices may create some activity, and induce orders to be given, and it is also probable that the large number of vessels that have been quite recently taken up may have some effect locally—more locally than generally, I am afraid—upon the freight market. No less, I believe, than one hundred vessels have been taken up and chartered by the Government for war purposes; but I cannot give you better evidence or better proof of the depressed condition of the freight market than by just reminding you of the prices at which these vessels, many of them the very pick of our great steam companies' fleets, have been taken up—namely, at 17*s.* 6*d.* per ton per month, whereas, in 1882, vessels were chartered by the Government at 28*s.* per ton per month to perform almost precisely the same services. That will give you a pretty good illustration, as I have said, of the depression in the freight market; and I think cargo steamers have been taken up also for the conveyance of railway material at the low rate of 10*s.* 6*d.* per ton per month. Now, naturally, I think, it will suggest itself to men's minds how is it, why is it, that manufacturing industry is at a low ebb, and has been for many months past, all over the civilised world.

Gentlemen, surely our common sense tells us that it must be owing to the unsettled condition of the civilised world. These diplomatic disputes, these disputes about boundaries, and these international difficulties that are arising, and are a very extraordinary feature of the present age—the rage that seems to have seized on the nations for colonization; war, real and prospective, these are all highly detrimental to commerce, because they are destructive of confidence, and confidence is the soul of commerce. With reference to these questions, of course I do not attempt to go into them; I merely express my firm conviction that they are, in the main, the cause of the depression of trade all over the world.

We have already heard, Gentlemen, within a very few days, of war policies being effected, of difficulties arising out of the exercise of the right of search; and we have also heard that very sinister rumour—I trust that it may not be entirely founded on fact—of the possible transfer of British bottoms to foreign flags.

Well, these are all matters that are incident to war, and it appears to me that although a temporary,

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and I trust it may be more than temporary, activity may be evinced in some quarters, yet as long as the *fons et origo mali* remains—namely, the fear of war, and the actual existence of war, it is really unreasonable to expect anything like a general improvement in the world's trade. All we can do, Gentlemen, is to hope for the best, and be prepared for the worst. There is, at least, one feature of a satisfactory character in the events of the past year, and that is the increasing interest taken by the nation in its naval affairs. The nation, I hope, has awakened at last from an apathy with regard to its navy, which has been to me a matter of profound astonishment for some years past, and I think I shall be able to show you the effect of that greater attention paid by the public to its naval affairs. In the spring of last year—it was just about this time last year—the Admiralty appear to have entertained some misgivings that our naval strength was not equal to the services that it might be called upon to perform, and also that the rate of construction of our war ships was unnecessarily slow in the dockyards, and they accordingly appointed a Committee of Inquiry into two subjects; the first was the mode in which Her Majesty's ships are built by contract, and the second branch of the inquiry was the mode in which Her Majesty's ships are repaired and fitted in the dockyards. Well, I am bound to say, having had the honour of being connected with that Committee, that in my humble judgment—with one glaring exception, which naturally occurs to every man's mind, namely, my own self—it was a Committee eminently qualified to perform the duties it had in hand. I say that out of respect to the colleagues with whom I was associated. I do not think you could easily have found, with that one exception, a body of gentlemen (because it was *not* a large Committee) more competent to enter into an exhaustive inquiry upon those two subjects. That Committee reported, about the end of the summer, I think in July. It has been thought by some persons that the inquiry was not sufficiently extensive. I am not at all sure, Gentlemen, that some members of the Committee did not entertain that view themselves; but I should like to point out to you that the Committee was absolutely bound by the terms of its order of reference, and it is not the practice—in fact, it is never done—for either Commissions or Committees to exceed the terms of their order of reference; but I am very far indeed from saying that I do not think such an extended inquiry would be necessary. I think it would be highly desirable for the interests of the country that any further inquiry should be of a very much more extended character. I think it would be desirable, but I must point out this—that it would require either a Commission or a Committee to be very differently composed, inasmuch as any such extended inquiry as might be contemplated would involve such questions, probably, as the constitution of the Board of Admiralty, the policy of the Board of Admiralty, the whole administration of the dockyards, and the supply of naval ordnance. I think I have said enough to show that the field of the inquiry would be a very large one, and a very difficult one to conduct, whilst it would require, as I have said, a very differently composed Commission to undertake such extensive duties as those. The Committee reported, as I said, towards the end of the summer, and I should like, if you will permit me, Gentlemen, if I am not occupying your time at an unreasonable length, to point out to you the policy which obviously guided the Committee in its main recommendations. The policy was simply to induce the Admiralty to avail themselves, to a much larger extent than they had hitherto done, of the resources of our private establishments. But in order, to ensure the co-operation of the able and eminent men who conduct those establishments certain conditions are necessary, and one is that they should have ensured to them a certain continuity of work, in order that the managers, the heads of those establishments, on their part shall be encouraged to retain in their service a body of workmen accustomed to do Admiralty work, and not only to do Admiralty work, but

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accustomed to Admiralty inspection; because Admiralty work, I need hardly tell you, differs widely from ordinary work, and the inspection being conducted on behalf of the nation is necessarily a very strict inspection.

It is also necessary that in order to avoid delays, and in order to frame accurate estimates, the designs, and the specifications for carrying out those designs, should be as clear, as definite, and complete as possible. That would enable more accurate estimates to be framed. And it is further necessary, I think, if the working of contracts is to proceed smoothly and expeditiously, which everybody must desire, that the contractors, in conjunction with the Admiralty, should come to a thorough agreement before the work is commenced as to what the work is really to be and how it is to be done. If that were so, many causes of delay would be avoided, and the work of the inspectors would, I believe, be confined to two points, namely, the inspection of the material and the inspection of the workmanship. The contractors having thus come to that thorough understanding with the Admiralty as to what they wanted to be done, might be left a larger latitude of discretion in carrying out small, and very often insignificant, details in building vessels. The difficulties, Gentlemen, that arise now—and they are very great, and cause very great loss of time, and I need not tell you that loss of time means loss of money—arise constantly upon matters of detail. Drawings are called for; they are framed by draughtsmen in the contractor's establishment; they are sent up to the Admiralty, and they are often detained there a considerable time, and returned sometimes with not very extensive, but trifling alterations, and sometimes with no alterations at all. But I need not point out to you that all this is productive of great delay, and if the recommendations of the Committee are to be carried out—and I am hoping to see the Admiralty carry them out—considerable smoothness and expedition in the construction of war ships by contract will probably be the result. It is also, I think, necessary, Gentlemen, that in controlling work of this kind, and superintending the construction of contract built vessels, very great care should be taken in selecting the overseers, that their standing and experience should be such as to command the respect of the very able and eminent men with whom they are brought into contact in these private establishments. I am not saying a word against the gentlemen who conduct that work, but with reference to these inspectors it undoubtedly would be a very great advantage to them, and to contractors themselves also, if they had more frequent opportunities of revisiting the dockyards in order to keep themselves, as it were, abreast of every improvement in the workmanship and carrying out of details which are now, as you know, extremely complicated; because, undoubtedly, what weighs upon the mind of the Admiralty inspector, when resident far from the Admiralty, is the expectation of the amount of criticism that his work will receive at the hands of the dockyard authorities, when the ship is sent round to be fitted. That necessarily weighs upon his mind, but had he more frequent opportunities of revisiting the yards he would know precisely what was going on in those yards, and he would be able to work up to the standard. These are matters dry, but still of very great importance, and I am very glad indeed to think that the Admiralty has shown a recently sincere desire to carry out the recommendations of the Committee upon these points.

I must now allude to the course the Admiralty took in the Autumn. The First Lord of the Admiralty towards the close of the Autumn Session—as you know, Parliament was called together for a very special purpose, but I think it was on the 2nd of December last—the First Lord of the Admiralty came down to the House, and made what I venture to say is the most important announcement that has

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been made in my recollection upon naval affairs, and he accompanied his announcement with a very extensive shipbuilding programme. I should like just to quote the words that were used by the First Lord himself on that occasion. In the early part of his speech he said that the announcement he was about to make was the outcome of the state of public feeling that had arisen during the recess in regard to naval questions. Well, the programme, Gentlemen, was a very extensive one, the largest that I ever remember (and I have been a long time in Parliament) being presented at one time to the nation. It contained a proposal for the expenditure of £3,100,000 to be spread over five years, in the construction of war ships, to consist of four iron-clad vessels; five very powerful steel cruisers—armoured cruisers, provided with belts of steel of no less than 10 inches, completely belted; two torpedo rams of the *Polyphemus* type; ten torpedo cruisers—sea going fast torpedo cruisers; and thirty first-class torpedo boats. With the exception of two of the iron-clad ships, which are to be laid down, one I believe at Portsmouth—the design of which is before Parliament at this moment, because it is exhibited in the House of Commons—and another to be laid down at Pembroke, and with the further exception of the two torpedo rams of the *Polyphmeus* type (of which I am sorry to say we have heard nothing since the programme was presented to the country), with the exception, I say, of those four vessels, it is proposed to build the whole by contract. Not only that, but I think the Constructive Department of the Admiralty deserve very great credit for the pains and the rapidity with which they have framed the designs and the specifications, involving as they do great detail, much greater detail than formerly, for the building of these vessels. Tenders have been received for, I think, six of the *Scout* class, which are the torpedo cruisers, and for a considerable number of the gunboats. For the two armour clads to be built by contract tenders have been invited; but not, I think, as yet received, and I do not think that the tenders have yet been returned for the steel-belted cruisers. The proposed tonnage to be built by contract has been increased from 4,614 to 11,338 tons; that is the amount of tonnage of ships of war proposed to be constructed by contract in the year which we are just entering upon.

Now, Gentlemen, there is another event which occurred in the Autumn, to which I wish to make just a passing allusion, and that is this: You, no doubt, have observed, and followed with great interest, the controversy which has arisen upon the matter of the type—the recent type, of our war ships, and which has been conducted with great energy, and has excited, no doubt, throughout the country very deep interest. Now, do not be alarmed, Gentlemen; I am not going to rush into it, because my opinion would be worthless; it would be the height of presumption and folly were I to attempt to pass any opinion upon the subject, but I wish just to make one or two observations in reference to it, because I do entertain a very strong conviction that public opinion will not be satisfied to leave the settlement of this question—the question, namely, of the citadel type of ship with the unarmoured ends *versus* the armour-belted ship—that is the controversy. I am sure that public opinion will not be satisfied to allow the Admiralty to be the sole judges in their own case in a matter of this kind. It affects not only the six ships that are now building, but it affects five others, that is eleven in all, beginning with the *Inflexible*, the *Ajax*, the *Agamemnon*, the *Colossus*, and the *Edinburgh*, and the six ships of the Admiral class. Now, it does so happen that the whole of these vessels, with the exception of the *Agamemnon*, are at home at present. This controversy is not a new one, as you know, and as I know also, because I happened to be mixed up with it many years ago in the House of Commons, and took a very deep interest in it; and therefore it is that I wish to make a few observations. The controversy arose

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originally not upon the design of the *Inflexible*, but upon the *Inflexible* when she had advanced a considerable way towards her completion. Sir Edward Reed, who has been perfectly consistent from the beginning in his objection to this class of ship, is now supported (and this is the point I want to come to) by a very powerful and influential array of naval opinion in his objections. Now, I want you to look for a single moment at the position of the naval officer with regard to this question. He says, as English officers have always said, "I am ready to fight your battles; I will go to sea, if you choose to send me to sea, in a cock boat; but I do not like this type; my naval experience and my knowledge of gunnery tell me that if these unarmoured ends were reduced to a complete state of destruction by the action of ordinary guns, or the fire of the machine gun which has come into use within quite recent years, the vessel would be in such a position that I could not control her." That is the important point; that is the doubt that exists in the minds of many naval men whose opinions ought to command respect: and I want to know whether some effort ought not to be made to remove those doubts. It is very difficult to bring this question to a test, because we cannot expect a Government, an economical Government at any rate, to send one of these vessels to sea and shoot her ends off, in order to see whether she will sink, or swim, or steer after the operation; but surely, Gentlemen, this is one of the questions which is essentially one to refer to independent opinion. Why not call in a council of advice upon such a matter as this, affecting no less than eleven of the most powerful ships—represented, at least, to be the most powerful ships—of our Navy? There are precedents for this. It occurred in the case of a vessel of Sir Edward Reed's own design long ago—upon the design of the *Devastation* and the *Thunderer*. On that occasion an independent Committee of inquiry was formed, and it was called Lord Lauderdale's Committee, and our old and esteemed friend, Dr. Woolley, was a member of that Committee. It occurred again, as I said before, upon the half-completed hull of the *Inflexible*, and I remember well the discussion in Parliament upon it. In that case Admiral Hope presided over the Committee, and again Dr. Woolley was a member of it. Now, I am merely suggesting this. I am not expressing an opinion one way or the other, but I am convinced that the country will require something in the nature of an independent decision upon this type of vessel. Such an independent tribunal of course would be composed of naval officers commanding the entire confidence and respect of the service, and, on the other hand, of Naval Architects commanding, from their position and experience, the entire confidence of the country at large. But it should be an independent body of gentlemen, unbiassed one way or the other, merely to decide from their own mathematical knowledge, and from their own experience and skill and ability, whether or no this is the best kind of fighting ship that can be built. That really is the question, because supposing the worst to come to the worst, and these vessels were adjudged not to be suited for the line of battle, they would be most useful in other ways. Nobody complains of the vessels; they would be of inestimable value to this country if it were only as guard-ships upon our distant lines of communication in any sea; from their speed (they run 16 knots an hour), from their prodigious offensive powers, they would be invaluable; nothing could escape them except an *Esmeralda* here and there. Therefore their value as portions of our fleet would be, as I said before, inestimable. The question resolves itself to the narrow point, What is the proper place of these vessels? And I do think that it would be desirable, in order to smooth down any heat that may arise over this controversy, that at least the type should be submitted to something like an independent council of advice, whose opinion would, unquestionably, have very great weight with the country and with the Admiralty itself. There need be no loss of dignity in this matter. I have shown you that there have been precedents

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for it, and as to insinuating blame for a single moment to the Constructive Department, I am the last man in the world to do that. I do not imply it in anything that I am saying at this moment. On the contrary, I object in the strongest way to attacks being made upon the Constructive Department, and Sir Edward Reed never said a truer word in his life—and he has said a very great many—than when he said, "I object to the Naval Architect being lugged on all occasions into these discussions." The Naval Architect, after all, is a gentleman who does what his employer asks him to do, and tells him to do, whether it is a private employer or a public employer. He says, "Tell me what you want; I will tell you what I can do; I will tell you what I cannot do. But tell me what you want, and I will design the ship for you, upon my experience, upon my mathematical knowledge, upon which my reputation very largely depends." That is the position of the Naval Architect, and if the ship happens to be not all that is expected of her you have no right to blame the Naval Architect; you must blame the man who ordered the ship and who called for the design, at least so it appears to me. Now, I want to prove, from the mouth of a Member of the Board of Admiralty himself, upon the question I have put: "Is this the best type of ship for the line of battle?" that he also shares in my opinion. The Secretary of the Admiralty said only the other day, on the 15th March, in a very able speech which he addressed in moving the Naval Estimates, "A great service like ours demands perfection, and is sensitively jealous of the slightest superiority." Well, nobody has ever said more than that, and the question is, Is this the most perfect type of ship? and that I say is a question upon which the country has a right to be satisfied.

Now, Gentlemen, we must be charitable, and we must remember that the Admiralty themselves have laboured under very great difficulties of late years in war-ship construction. There have been delays, unaccountable delays, but, at the same time, they have had to contend with two great difficulties; first of all, the introduction of compound armour, and, secondly, the substitution of the breech-loading gun for the muzzle-loading gun; and this question of the guns is really the question that lies at the bottom of the delays and the difficulties, of which the outside public are complaining, and with some reason, that occur in the completion of our ships of war. No less an authority than the Director of Naval Construction said in my presence this last year, "If we only knew exactly what the gun was to be, and "its charge, at the time of designing the ship, we could build any iron-clad in three years." Now we all know that iron-clads have taken six years to build, and I myself saw last year a magnificent ship apparently finished lying in the basin at Chatham; she was the *Conqueror*. We asked when the ship was going to be commissioned. "Oh! she is waiting for the breech screws of her big guns." And I am told, Gentlemen, that she is waiting there still for the breech screws of her big guns—the ship complete in every respect, her guns on board, and she is waiting for the breech screws! Now that cannot be right, and consequently I am extremely rejoiced, and I am sure you will be rejoiced, to hear that quite recently, within the last few days, a Committee has been appointed by the Admiralty to inquire into this question of the big guns; because it is really one of the most important questions in regard to ship construction that can by possibility be considered. The 110-ton gun, I understand, takes fifteen months to make, about half the time that would be required to build an iron-clad by contract. And in view of two or three most important considerations, an exhaustive inquiry, both into the supply of our great guns and of the material that composes them, is, I think, absolutely necessary and fair by the country; because they want to be enlightened upon certain points, in view of the inordinate length of

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these guns, their unwieldy length; in view of their immense weight, which necessitates their being placed high in the ship; in view of their prodigious explosive powers (and it is contemplated to put two of these guns in the same turret); in view, last of all, but certainly not least, of their prodigious cost; and in view, likewise, of the risk and difficulty attending the striking of a moving object at a great distance—at their full distance—which would inevitably weigh upon the minds of officers. I hope the Committee will ascertain the broad fact, whether these monster guns are necessary for sea fighting. I think it demands the very best consideration, and I trust that it will be given to it. There are other matters connected with the supply of the material, the steel ingots, which is a most important question, and I am very happy to hear in reference to that matter that at least two most eminent firms at Sheffield are at this moment engaged in laying down the plant, the costly and extensive plant, for the purpose of making the steel ingots necessary for our gun manufacture. But, Gentlemen—this is almost the last remark I have got to make—the time has come, in my humble opinion, when the country has the right to expect the Admiralty to be responsible for the supply of its own guns for its own ships, altogether independent of any other Public Department. Until that is done my belief is that these disappointments in the fulfilling of the programme of the year will occur, and we shall have to face the difficulties that have arisen for many years past, not so much in the building as in the completion of our ships of war. Gentlemen, I am extremely grateful to you for the kindness with which you have listened to my undue tax upon your patience; I feel that I owe you many apologies. I thank you most cordially for the kindness with which you have listened to me.

After the reading and discussion of Mr. P. Watts' Paper, the Scrutineers appointed to examine the Ballot Papers for the Election of the New Council presented the following Report:—

Society of Arts, John Street, Adelphi, London, *W.C.*,
March 25th, 1885.

To the President of the Institution of Naval Architects.

My Lord,—We have the honour to inform you that having gone through the voting papers for the Election of Members of Council and Associate Members of Council, that we find the following are duly elected:—

Denny, W.	Kirk, A. C.	Rennie, G. B.
Dunn, W.	Laing, J.	Sennett, R.
Elgar, F.	Laird, H. H.	Thomson, J. R.
Gray, J. Mc F.	Marshall, F.	Thornycroft, J. I.
Green, H.	Martell, B.	Watts, P.
Harland, E. J.	Morgan, H.	Waymouth, B.
Humphrys, R. H.	Parker, W.	West, H. H.
Inglis, J., jun.	Pearce, W.	White, W. H.
John, W.	Price, J.	

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Associate Members of Council elected as follows:—

Ailsa, Marquis of.	Froude, R. E.	Ramsden, Sir James.
Cotterill, J. H.	Kemp, Dixon	Rundell, W. W.
Cowper, E. A.	Noel, G. H., Captain.	

The total number of voting papers was 113.

We have the honour to be,

Your Lordship's obedient Servants,

GEORGE B. RENNIE,
GEORGE COLBY MACKROW.

Since the issue of Vol. XXV. the following Gentlemen have been added to the lists of Members and Associates respectively:—Messrs. Henry H. Ash, Sydney Walker Barnaby, John Black, Arthur W. Blake, James S. Clark, Victor Daymard, Frederick Edwards, Neville Evans, Edward Faron, John H. Heck, William Laing, James H. Morgan, John Mowat, Alexander T. Orr, Peter Phorson, Pierre Piaud, John S. Pirrie, S. Popper, James Readhead, Norman Selfe, Pablo P. Seoane, John Stewart, Alfred Oscar Trygger, Saul Warner, Walter Henry Wilson, Robert Wyllie, Members; and Messrs. Washington Irving Babcock, François Barry, Peter Blair Black, Samuel Richardson Blundstone, Alfred James Corry, William Cowie, Captain C. C. P. Fitzgerald, Thomas Gray, Arnold F. Hill, Captain F. Labrano, Joseph Hamilton Linnard, William George Neal, Harold Percival Norton, J. M. H. Taylor, Ignatius E. Thayer, and Joseph J. Woodward, Associates.

During the same period the Institution has sustained the loss of the following Members and Associates:—James Webb and Henry Wimshurst, Members, and Frederick Wilkinson, Associate.

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PAPERS AND DISCUSSIONS.

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A PRACTICAL MEASUREMENT OF THE COMPARATIVE FIGHTING EFFICIENCY OF SHIPS OF WAR.

By Captain G. H. Noel, R.N., Associate, Member of Council.

[Read at the Twenty-sixth Session of the Institution of Naval Architects, March 25th, 1885; the Right Hon. the Earl of Ravensworth, President, in the Chair.]

In preparing a Paper of this description, I am fully alive to the difficulties to be contended with, and I must apologize to the scientific gentlemen present, should the result of my efforts appear crude and incomplete. A practical means of estimating the comparative fighting efficiency or value of ships of war, has long seemed to me to be wanting; and of late so much has been said, and written, on the comparative power of the fleets of Europe, that this want in my opinion has become more apparent.

In 1878, M. Marchal of the French Navy, published in the *Revue Maritime* some most elaborate and minutely worked-out tables for classing ships of war, but his somewhat complicated, though very exhaustive system, does not appear to have come into use in this country. When his article first appeared I studied it, in the hope of finding that it would facilitate the working out of like comparisons, but soon realised how great would be the difficulty to acquire the data necessary to work on a system so full of minute details. I have since thought much on the subject, but it is only quite recently that I have committed my views to writing, partly I may say on account of some comparisons, with regard to armoured tonnage, which in my opinion did not increase the value of Sir Edward Reed's very powerful lecture delivered in November last, on "The most urgent measures to be "taken for increasing H.M. Navy," in which they appear.

I am aware that there may be no generally felt need for such a means as this of comparing the efficiency of ships of war, otherwise it would probably have been brought before this Institution, and treated by far abler hands than mine: nor should I have undertaken to read this Paper were it not that by doing so, I gain the opportunity of offering my opinion as a naval officer, on the various qualifications of a ship of war.

With the benefit derived from a study of M. Marchal's Paper, and the kind assistance

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of some of the gentlemen present, I have been able to take the work in hand, and in compiling this table have endeavoured to combine easily acquired data and simplicity of calculation, with the full appreciation of all the different qualities which render a war ship valuable. The co-efficients I have used are of necessity my own, and I should be indeed presumptuous if I expected them to be generally accepted; arguments may convince me of the desirability of their modification. The plan which is here submitted with a few explanatory remarks, is intended to enable a comparison to be made between all classes of vessels, from the heaviest armour-clad, to a first-class torpedo boat.

The formula which I have adopted is as follows:—

$$\text{Figure of merit} = \alpha + \beta + \gamma.$$

where α represents a number of qualifications (with co-efficients in some cases slightly differing to suit the circumstances), most of which are considered to vary directly as the elements they express; the remainder varying as the displacement, or some power of the displacement.

Where β represents another set of qualifications, all of which are considered to vary directly as the square root of displacement.

And where γ represents a miscellaneous set of qualities or defects which apply chiefly to special types of vessels.

I now propose to give a short explanation of all the items, taking them in the order in which they stand in the formula. First as regards those contained in α.

$$\alpha = a\,A + b\,B + c\,C + d\,D + g\,G + H + L + S + T.$$

Armour.

A = Total weight of armour and backing in tons.

$$\text{Coefficients of A, or } a = \begin{cases} \cdot 06 \text{ if armour is of wrought iron.} \\ \cdot 07 \text{ if armour is of steel or is steel-faced.} \end{cases}$$

Armour is here given value according to the total weight carried, irrespective of thickness or position; steel or steel-faced armour, being considered 16·$\dot{6}\dot{6}$ per cent. more valuable than wrought iron.

There is at the present time much controversy about the position of armour, and it appears likely that no definite conclusions will be arrived at, until the matter has been fully

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tested in battle. I am inclined to think there is reason for both the citadel and the water-line form of armour-plating; of these two types the citadel ship will be found best against heavily armed forts, where in the absence of the enemy's rams, she might be taken out of action for repairs should her ends be so damaged as to hamper her manœuvring power. Whereas a belted ship with less thickness of armour, will be more suitable for meeting the foe in blue water, where she can afford to scorn the uncertain fire of a few heavy guns, and will be sufficiently protected to maintain her water-line intact against a ship armed with moderate-sized guns. As there are enough ships afloat under the flags of possible enemies to occupy the whole attention of our fleet, I begrudge the employment of our ironclads against forts (unless such an action is unavoidable); and I give preference to the water-line armour in contradistinction to the armoured citadel. Still, assuming that the workmanship is equally good in all ironclads, and that the safety of the ship is not imperilled by the position of the armour, I am fully persuaded that the manner I propose of apportioning value to armour and its backing, according to weight only, is the most reasonable.

My objections to the recent citadel type of ships do not coincide with those of Sir Edward Reed, who, in his letter to the *Times* of February 16th, would apparently condemn them on almost every account. If machine guns are to do the infinity of damage suggested by him, there will be no living in any part of a vessel that is not armoured with 2-inch steel, or (since it is now proposed to have 36-pounder quick-firing guns), even a greater thickness. I am not sure that machine guns will be ever aimed at the water-line of a large ship known to be partly protected by armour; but, whether this is or is not probable, I quite agree with Sir Edward Reed, that a certain thickness of plating is desirable to prevent small shot from penetrating any part of the water-line of an armoured ship, and I am of opinion that if such a ship as the *Collingwood* were belted with 3-inch steel, where her water-line is now only of thin iron, she would, so far as armour-plating is concerned, be as serviceable a vessel as any afloat. About 150 tons additional weight in 3-inch steel plating would be sufficient to give the *Collingwood* a 6-feet water-line belt at her unarmoured ends; this would add about 4 inches to her immersion. Even a 2-inch steel belt would afford a great additional protection.

It must be borne in mind that armour is only a protection against guns; that guns form little more than half the offensive power of a well-designed ship, and hence great weight of armour is out of due proportion, especially as the small experience we have of gun power in action, indicates that even unarmoured ships are seldom sunk by artillery alone.

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My two principal objections to the first type of citadel ships were, the confined nature of the battery, and the height of the heavy side and turret armour, causing a high centre of gravity in a vessel whose reserve of buoyancy might be so greatly reduced in action as to create a liability to capsize. In the *Collingwood* both these objections are obviated; her armour is low, giving greater stability, and her heaviest guns, "*en barbette*," are outside the citadel, leaving plenty of space for other guns, towage of boats, &c.

Battery of Guns.

B = aggregate weight of all guns in tons (not including machine and quick-firing guns).

Co-efficients of B, or $b = \begin{cases} \cdot 5 \text{ if guns are of wrought iron, or old construction.} \\ \cdot 6 \text{ if guns are of steel, or principally of steel.} \end{cases}$

It is generally acknowledged that steel will take the place of wrought iron in the manufacture of all guns. I am not altogether satisfied that we have yet sufficiently overcome the treachery natural to steel, when exposed to sudden and enormous strains and variations of temperature, to ensure its safe use in the building up of the heavier nature of guns; but the fact remains that for the same power steel guns are lighter than wrought iron, for which reason they are here given 20 per cent. the advantage of older ordnance. The armament is treated in the same manner as the armour, and is given value according to total weight, irrespective of the position or nature of the guns carried. Batteries vary so much in form, according to the views of the constructive department, and the popular plans of the day (often without any apparent benefit being derived from such variations) that it would be impossible to give a satisfactory value to the many items composing the artillery power of a ship, and when the *pros* and *cons* have been duly considered the result reduces itself into very much the comparative value I have adopted above.

Perhaps it may be thought that a fairer measure of this power would be obtained by substituting the aggregate energy displayed (at muzzle in ft. tons) of all the guns, for their weight, and dividing the result by 650, or some co-efficient which would give it a reasonable value, as compared with other elements. I should have adopted this plan but for two objections: first, muzzle energy is only a true measure in comparing armour-piercing guns; and secondly, this energy is dependent on the weight of projectiles and power of charge, both somewhat variable quantities, subject to difference in nature of shot, improvement in powder, and unforeseen weakness exhibited in the gun which would necessitate a reduction in the charge.

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The question of giving additional value to the guns protected by armour has been purposely disregarded, as such a protection varies so exceedingly in its nature as to be very difficult to deal with, and is after all not an unmitigated benefit.

Coal.

C = Total weight of coal carried (in tons).
Co-eff. of C, or $c = \cdot 05$.

This measure includes the weight of all the coal carried, or as it is commonly called "the capacity of bunkers," often a very different amount to the "complement of coal."

The value of fuel cannot be doubted, and it is greatly to be deplored that the bulkiness of coal should so limit its supply. One of the greatest difficulties with which we shall have to contend in time of war, will be that of keeping our ships supplied with coal. It will then be imperative that squadrons should be accompanied in all their movements by colliers; and until we can find a substance that will give us the power of steaming double the distance, for the same bulk as coal, it would be the height of folly to send cruizers into distant waters in war time, dependent solely on their own coal supply, for their motive power.

Displacement.

D = Displacement of ship in tons.

$$\text{Co-effs. of D, or } d = \begin{cases} \cdot 009 \text{ if hull of ship is of wood.} \\ \cdot 01 \text{ if hull of ship is of iron or composite build.} \\ \cdot 011 \text{ if hull of ship is of steel.} \end{cases}$$

Displacement is here given a definite value, as without question the size of the ship, must considerably influence such a comparison as this. The co-efficients have different values, according to the material of the hull, advantage being given to steel over iron, as in vessels constructed of this material there is saving of both weight and space (which can be applied to other components of the total displacement), and to iron over wood, for similar reasons.

In an article on the Navy, in the *Encyclopædia Britannica*, Mr. Barnaby suggests that tons displacement is in itself a good measure for comparing ships of war of the same date. I had previously come to a similar conclusion, and in arranging some Tables of Foreign Ships of War, for Captain Bedford's *Sailor's Handbook*, 1883, I classed the

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ships according to their age, and placed the tonnage and date of launching immediately after the list of names, as being the principal elements for consideration, in drawing a comparison between the fleets of different nations.

Where all the various fighting qualities of a ship are given places, as in this Table, it may be thought unnecessary to introduce tonnage as having a separate value in itself: but as all classes of vessels are here included, I find it indispensable as a means of adjusting the values of unarmoured ships (including armed merchant ships).

Machine Guns.

$$G = \left\{ \begin{array}{l} \text{Aggregate weight of machine and quick-firing guns in cwt.,} \\ \qquad \text{not including heavier guns than 6-pounders.} \end{array} \right.$$

Co-eff. of G, or $g = \cdot 1$.

The importance of machine and quick-firing guns is such, that they are given three to four times as much value as a similar weight of the ordinary guns of a ship's battery. Only one co-efficient is now quoted, but if 36-pounder quick-firing guns, and others of a heavier nature, be introduced, another co-efficient (of ·08 or ·05) would have to be added to express them, as it would be unreasonable to attach to a quick-firing gun more than double the value of other ordnance of equal weight.

Complement.

$$H = \frac{26\,h - D}{200} \text{ where } \left\{ \begin{array}{l} h = \text{complement of officers and men.} \\ D = \text{tons displacement (as above).} \end{array} \right.$$

This formula appears rather involved, but I could find no other expression which would give the value I desired.

Of late years the importance of a full and efficient crew has been somewhat lost sight of. Modern ships are wanting in accommodation, and as they carry so much machinery, less manual work is required; but nevertheless, men in numbers are always of value, and may be invaluable on certain occasions. We must not forget the services rendered by the *Shannon's* brigade during the Indian mutiny, and the *Shah's* brigade in Zululand, nor the detached work seamen will so often be called upon to perform in landing parties, for river work or torpedo operations, and as prize crews.

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Lightness of Draught.

$$L = \frac{D}{2{,}000}(27 - l) \text{ where } \begin{cases} l = \text{draught of water aft at load displacement.} \\ D = \text{tons displacement as (above).} \end{cases}$$

Lightness of draught is an undoubted benefit, to obtain which, in some cases, much has been sacrificed. The number 27 is selected as being in excess of the extreme draught of most modern ships; where the draught is greater than 27 feet, as in the case of the *Lepanto*, L becomes a minus quantity, for such a deep draught detracts greatly from the general value of a ship of war, by limiting her area of action.

Speed.

$$S = \frac{D^{\frac{2}{3}}}{250}(s - 8) \text{ where } \begin{cases} s = \text{full speed of ship in knots at trial on measured mile.} \\ D = \text{tons displacement (as above).} \end{cases}$$

This formula will, I think, give due weight to the great advantage superior speed carries with it. For instance, given three 10,000 ton ships with 8, 16, and 18 knots speed respectively, the marks awarded would be 0, 118, and 186.

In determining this formula I was obliged to make the speed vary, as the displacement of the ship to the power of two-thirds, in order to apportion a fair value to all vessels (thus introducing, against my wish, another power of displacement). If "speed" was transferred to β whose terms vary as the square root of displacement, too much value would be given to the high speed of small vessels. In all cases the actual measured mile speed which a ship is capable of attaining at the time of making this calculation should be used, so that allowances must be made if the boilers are worn and their pressure reduced since the last trial.

Torpedoes.

$$T = tD^{\frac{1}{4}} \text{ where } \begin{cases} t = \text{No. of carriages or tubes for discharging torpedoes from the ship.} \\ D = \text{tons displacement (as above).} \end{cases}$$

Torpedo armament is perhaps more suitable to small vessels than to large, but considering that size carries with it certain advantages, such as greater working room, and steadiness of platform, slight additional value is given to the latter by making the term

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vary as the fourth root of displacement. No difference in value is attributed to the discharge, whether from submerged tubes or from carriages above water. The former method, if perfected, would be of far greater value than the latter, but so many difficulties have hitherto arisen to retard its being brought to perfection, that it is doubtful if it will ever be generally adopted. This concludes the items comprised in α.

$$\beta = \frac{\sqrt{D}}{100}(M + N + O + P + R + W)$$

All the terms in β are multiplied by $\frac{\sqrt{D}}{100}$ and in this manner vary as the square root of displacement.

Manœuvring Power.

$M = 70 - \frac{m}{10}$ where m = diameter of circle turned at full speed, and using all helm, in yards.

Exact information on turning power is somewhat difficult to obtain, but the diameter of the circle turned can be estimated with a fair amount of accuracy, by comparing the length and beam of any ship, with that of a vessel of somewhat similar dimensions, whose turning power is known. The diameter of the circle turned by the most unwieldy war ship, should not exceed 700 yards. I have therefore taken this as the measure from which to calculate value.

Sail Power.

$$N = \frac{A}{3\,D^{\frac{2}{3}}} \text{ where } \begin{cases} A = \text{area of plain sail in square feet.} \\ D = \text{tons displacement (as above).} \end{cases}$$

The above commonly accepted formula, but in this case divided by three, appears to be very suitable. Sail power has lost much of its value, but owing to the bulky nature of coal, it is still indispensable as a supplement to the fuel of such of our cruizers and smaller vessels as are required for service in distant waters. It has been said by a high authority in such matters that a *mastless* cruizer, in order to carry sufficient fuel, must be of 10,000 tons displacement. Under present circumstances I believe such to be certainly the case; therefore since we must have smaller cruizers, it is only reasonable to assign to sail power a fair proportion of value.

I know I have many opponents on this question, and that one who advocates sail power is stamped as being behind the times; this led me to write a Paper on the subject

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(which will be found in the *Royal United Service Institution Journal*, No. 121, 1883) in which I maintain that sail power is still necessary for *all* ocean cruizers, and that instead of dismasting them, a modified system of masting should be introduced, having for its chief recommendation that whilst capable of spreading a large area of canvas to favourable winds, it can be quickly and effectively reduced, so as to offer the least possible resistance when steaming head to wind, and the greatest snugness when going into action.

Efficiency of Conning Tower.

$O = p\sqrt{i}$ where

p = value of position and nature of conning station as per standard types	*Colossus*	15
	Inflexible, *Conqueror*	12
	Dreadnought	10
i = thickness of armour on tower in inches	*Alexandra* and older types	7

An efficient conning station with armoured tower to protect the steam-steering wheel, and means of communication with the different parts of the ship, is of very considerable importance. The most generally approved type in our service is that of the *Colossus*, to which the highest marks are awarded in the above Table; the other less satisfactory types are given value according to the difference in their points of merit. In all cases the value is made to vary as the square root of the thickness of armour.

Duality of Propellers.

P = The *sum* of the following values according to circumstances	Duality of propellers	20
	Duality of engine rooms	5
	Duality of boiler rooms	5

There is great security in having duality in the engines for propelling the ship, and if this is combined with separate water-tight engine-rooms and boiler-rooms, further advantage is gained.

Ram Bow.

R = value of Ram Bow as per following standard types	Modified *Polyphemus*	110
	Hercules	100
	Devastation and French types	90
	Inflexible, *Admiral* class and earlier ironclads	80
	C class Corvette	50
	Inconstant, and vessels with strong up and down bows	30
	Overhanging bows, and light built bows, such as *Mercury*	20
	Wood built ships	*nil.*

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In awarding value to the Ram-Bow, I may appear to be too arbitrary, for having in some cases no definite information, as to the internal structural strength of the work, I have been obliged to make the calculations from the form and outward appearance of the bow, and my knowledge of various systems of construction employed. But this matter is one involving so much technical knowledge that properly it should be decided upon by a committee of experts. I referred to this subject in a Paper read in this Institution in 1883, and there pointed out that the form which appeared to me to carry with it the greatest merit was one combining the cigar-end shaped ram of the *Polyphemus* (of course without the torpedo tube inside it) with the high stem of a sea-going ironclad; to this form, which is termed above the modified *Polyphemus*, I have awarded 110 marks.

In the above Table values are attached to other types for the following reasons:—

The *Hercules* has a high bow, with ram, pointed (but not too sharp), and growing out of the vessel in such a shapely manner as will enable it to resist very great lateral strain.

The *Devastation's* ram is powerful and backed up by armour, but comparatively low, and not so well supported by the upper part of the stem, as is the case in the *Hercules*. The French type is given the same value as the *Devastation;* it varies little in all their armoured ships, being generally a long protruding snout, supported by armour, and apparently firmly built into the ship.

The *Inflexible's* ram has a moveable point, and is unsupported by side armour, otherwise it is of good design. The *Admiral* class is given the same value as *Inflexible*, the ram being much of the same form, but on a sharper bow, which in my opinion is too slim to resist great lateral strain. The rams of the earlier types of ironclads are also given the same value as that of the *Inflexible*, as they are generally of powerful construction, and in most cases the ram-bow is supported by armour.

The C class of corvettes comes next on the list, and lastly a small value is attributed to both up-and-down and overhanging bows, as in well-built steel or iron ships of war both forms have a certain amount of power, to run down and sink vessels of like build.

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Seaworthiness.

$$W = \frac{w.z.r(6e+f+2)}{8n} \text{ where } \begin{cases} w = \text{angle of maximum stability.} \\ z = \text{maximum arm of righting lever (G Z).} \\ r = \text{ratio of reserve of buoyancy to displacement.} \\ e = \text{proportion of water-line protected by armour over 6″ in thickness.} \\ f = \text{proportion of water-line protected by coal, cork or light armour.} \\ n = \begin{cases} 3. \text{ for light built ships under 500 tons displacement.} \\ 2. \text{ for light built ships between 500 and 2,000 tons, and for strongly built ships under 1,000 tons.} \\ 1. \text{ for all larger ships than the above.} \end{cases} \end{cases}$$

This item involves so many considerations, that I have found great difficulty in expressing it with any degree of simplicity. The formula given combines, I hope, all the principal qualities which influence the seaworthiness of a ship of war. The three following points are taken into consideration, and treated according to their respective merits. (1.) A measure of the curve of stability (represented in the formula by $\frac{w\ z}{96}$). (2.) Reserve of buoyancy, the value of which is regulated by the amount of protection it receives at the water-line from armour or other means, {represented in the formula by $12\,r\,(6\,e + f + 2)$}. (3.) The structural strength of the vessel (as expressed by n). Such qualities as beam, freeboard, metacentric height, and others of minor importance, are absorbed in the above three points, and need no separate consideration.

At first it was my intention only to treat of seaworthiness in ships when they were intact; but the introduction of the element of protection to the reserve of buoyancy, recommended itself; and considering how necessary such a protection is (especially where the reserve is small), I do not think it is out of place, as the element introduces a value, varying according to the degree of damage to the water-line (and consequent loss of buoyancy) likely to be incurred in action.

This concludes the items comprised in β.

$$\gamma = U + V + X - Y.$$

The following items do not necessarily apply to all ships, but treat of special qualifications or defects, which ought to be recognised in such a comparison as this.

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Special Economy of Engines.

$$U = \frac{C(3-u)}{100} \text{ where } \begin{cases} u = \text{expenditure of coal in lbs. per I.H.P. per hour working at half-power.} \\ C = \text{tons weight of coal carried (as above).} \end{cases}$$

It is intended here that advantage should be given for special economy of engines, the outside limit being 3 lbs. per I.H.P. per hour. The element of "weight of coal carried," is brought into the calculation, as such an economy gives additional value to every ton of coal on board the ship.

Ships' (special) Torpedo Boats.

V = No. of Special (2nd Class or Turnabout) Torpedo boats carried by the Ship.

Co-eff. of V, or $v = \begin{cases} 10 \text{ in regular Ships of war.} \\ 5 \text{ in Torpedo Store Ships.} \end{cases}$

Extra value is here given to those ships of war, and torpedo store ships, which carry special torpedo boats, as their offensive and defensive power is thereby much increased.

Specialities.

X = Specialities, with values according to circumstances.

This column is inserted for use in cases where a vessel is fitted with any important speciality, which cannot otherwise be accounted for in this Table. The ballast of H.M.S. *Polyphemus* (capable of being dropped) is an undoubted speciality, but one to which I am not inclined to give much value.

Defects.

Y = Deduction according to degree of defect and tonnage of ship.	For defective steering	0 to 100
	Absence of ordinary water-tight compartments . .	0 to 30
	Presence of ballast contrary to design. Tons weight .	× ·05
	Inferiority of workmanship	0 to 100

Under this heading it is intended that those matters should be treated, which do not belong elsewhere in the Table. Such as defective steering, absence of, or insufficient number of water-tight compartments, presence of ballast in ironclads where it was not intended in the original design, and inferior workmanship; to which may be added any

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defects which exhibit themselves after a vessel is completed, and which cannot easily be rectified.

This last item completes the Table.

No reference has been made to certain points, which may be thought to merit attention, such as "Torpedo net defence," "Electric search light," liability of fouling, the habitability of the ship, and other minor matters, any of which, if considered of sufficient importance, may be added to this Table, under the heading of specialities or defects. Torpedo net defence is a subject which should demand notice, but as no definite practicable system has been yet adopted in our Service, perhaps it is better to leave the matter in abeyance.

The electric search light has, in my opinion, been made an object of too much importance; it must be admitted that such a light is useful on certain occasions; notably in entering a crowded anchorage on a dark night; in signalling; and in lighting up the foreground of a shore position; for these reasons it should certainly be supplied to ships, in the same manner as sounding machines, patent logs, and other useful articles. Its value in operations against the enemy afloat has yet to be decided, as it is questionable whether by exposing the ship's position it will not do more harm than good.

Deterioration has not been referred to; it is a difficult matter to treat separately, and except in the case of wood-built vessels, does not materially affect the whole question; an iron or steel ship, if kept in good order, will last for many years without exhibiting any structural weakness; her armament may be changed with the times, and if she does lose speed, through wear of engines and boilers, her value lessens under that heading.

I have endeavoured in this Paper to take an impartial view of my subject, and to avoid as much as possible controversial points; hence I have treated many of the items in a general, rather than a detailed manner. It is impossible to form a judgment of the value of a system such as this without applying it to practical use. I have therefore selected a few specimen ships as examples of its working, and have given the result of each item in full in the following Table. Comparison can of course be made between the various values attached to the different qualities of any given ship, as well as between the ships themselves. I much regret that want of time prevented me from obtaining a good deal of the information which is necessary to make the Table complete; many of the items are only estimated.

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Formula and Data required for comparing the Fighting Efficiency of Ships of War.

Figure of Merit $= \alpha + \beta + \gamma$.

Headings.	$\alpha = aA + bB + cC + dD + gG + H + L + S + T.$
Armour . . .	A = Total weight of armour and backing in tons. Co-effs. of A, or a = {·06 if armour is of wrought iron. ·07 if armour is of steel, or is steel faced.}
Battery . . .	B = Aggregate weight of all guns in tons (not including machine and quick firing guns). Co-effs. of B, or b = {·5 if guns are of wrought iron, or old construction. ·6 if guns are of steel, or principally of steel.}
Coal . . .	C = Total weight of coal carried (in tons), Co-eff. of C, or c = ·05.
Displacement . .	D = Total weight of ship's displacement in tons. Co-effs. of D, or d = {·009 if hull of ship is of wood. ·01 if hull of ship is of iron or composite build. ·011 if hull of ship is of steel.}
Machine Guns . .	G = Aggregate weight of machine and quick firing guns in cwt. not including heavier guns than 6 pounders. Co-eff. of G, or g = ·1.
Crew . . .	$H = \frac{26h - D}{200}$ where {h = Complement of officers and men. D = Tons displacement (as above.)}
Lightness of Draught .	$L = \frac{D}{2000}(27 - l)$ where {l = Draught of water aft at load displacement. D = Tons displacement (as above.)}
Speed . . .	$S = \frac{D^{\frac{2}{3}}}{250}(s - 8)^2$ where {s = Full speed of ship in knots at trial on measured mile. D = Tons displacement (as above.)}
Torpedoes . . .	$T = t\,D^{\frac{2}{3}}$ where {t = No. of carriages or tubes for discharging Torpedoes from the ship. D = Tons displacement (as above.)}
	$\beta = \frac{\sqrt{D}}{100}(M \times N + O + P + R + W.)$
Manœuvring Power .	$M = 70 - \frac{m}{10}$ where m = {diameter of circle turned at full speed and using all helm, in yards.}
Sail Power . .	$N = \frac{A}{3D^{\frac{2}{3}}}$ where {A = Area of plain sail in square feet. D = Tons displacement (as above.)}
Efficiency of Conning Tower	$O = p.\sqrt{i}$ where {p = {Value of position and nature of Conning Station as per standard types . .} i = Thickness of armour on tower in inches} {*Colossus*. . . . 15; *Inflexible* and *Conqueror* . 12; *Dreadnought* . 10; *Alexandra* and older types . 7}
Duality of Propellers .	P = {The *sum* of the following values according to circumstances} {Duality of propellers . . 20; Duality of engine-rooms . 5; Duality of boiler-rooms . 5}

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FORMULA AND DATA—*continued.*

Headings.	$\beta = \frac{\sqrt{D}}{100}(M + N + O + P + R + W)$—*continued.*
Ram-Bow . . .	R = Value of ram-bow as per following standard types . . . . . Modified *Polyphemus* . . . 110 *Hercules* . . . . . 100 *Devastation* and French types . 90 *Inflexible*, *Admiral* class and earlier ironclads . . . . . 80 C. class corvettes . . . . . 50 *Inconstant*, and vessels with strong up-and-down bows . . . 30 Overhanging bows, and light built bows, such as *Mercury* . . 20 Wood-built ships . . . *nil.*
Seaworthiness . .	$W = \frac{w\,s\,r\,(6\,e + f + 2}{8\,n}$ where w = angle of maximum stability. s = maximum angle of righting lever GZ. r = ratio of reserve of buoyancy to displacement. e = proportion of water-line protected by armour over 6 ins. in thickness. f = proportion of water-line protected by coal, cork, or thin armour. n = 3. For light built ships under 500 tons displacement. 2. For light built ships between 500 and 2,000 tons, and strongly built ships under 1,000 tons. 1. For all larger ships.
	$\gamma = U + v\,V + X - Y.$
Special economy of Engines	$U = \frac{C(3-u)}{100}$ where u = expenditure of coal in lbs. per I.H.P. per hour working at half-power. C = tons weight of coal carried (as above.)
Ships' (special) Torpedo Boats	V = No. of special (2nd class or turn-about) Torpedo boats carried by the ship. Co-eff. of V, or v = 10 in regular ships of war. 5 in Torpedo store ships.
Specialities . .	X = Specialities, with values according to circumstances.
Defects . . .	Y = Deduction according to degree of defect, and tonnage of ship For defective steering . . . 0 to 100 Absence of ordinary water-tight compartments . . . 0 to 30 Presence of ballast contrary to design . . . tons weight × ·05 Inferiority of workmanship . . 0 to 100

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FIGURE OF MERIT

Name of Ship.	Tonnage.	$a = aA + bB + cC + dD + gG + H + L + S + T.$									
		aA Armour co-effs. $a = \begin{cases} \cdot 06 \\ \cdot 07 \end{cases}$	bB Battery co-effs. $b = \begin{cases} \cdot 5 \\ \cdot 6 \end{cases}$	cC Coal co-eff. $c = \cdot 05$	dD Displacement co-effs. $d = \begin{cases} \cdot 009 \\ \cdot 01 \\ \cdot 011 \end{cases}$	gG Machine Guns. co-eff. $g = \cdot 1$.	H Crew. $H = \frac{26\,h - D}{200}$	L Lightness of Draught. $L = \frac{D}{2{,}000}(27 - l)$.	S Speed. $S = \frac{D^{\frac{2}{3}}}{250}(s-8)^2$	T Torpedoes. $T = t\,D^{\frac{1}{2}}$.	Total of a.
H.M.S. *Inflexible* .	11,880	3,400 tons 204·0	221 tons 162·0	1,300 tons 65·0	118·8	8·6	457 ·0	25·5 ft. 9·0	13·8 knots 70·0	4 41·6	679·0
H.M.S. *Dreadnought* .	10,820	3,880 tons 232·8	152 tons 76·0	1,450 tons 72·5	108·2	8·6	369 - 6	28·75 ft. 1·3	14 knots 70·5	2 20·4	584·3
Duilio (Italian) . .	10,486	3,140 tons 188·4	116 tons 208·0	1,280 tons 64·0	104·8	9·0[E]	400 ·0[E]	25·5 ft. 2·6	15·1 knots 96·8	4 40·8[E]	714·4
Amiral Duperré (French)	10,420	3,500 tons 245·0[E]	222 tons 133·2	1,000 tons 56·0[E]	113·5	9·0[E]	360 13·0	26·75 ft. 1·2	14·5 knots 81·1	4 40·0[E]	686·0
H.M.S. *Alexandra* .	9,490	2,340 tons 140·4	236 tons 118·0	680 tons 34·0	94·9	9·0	675 40·3	26·5 ft. 2·4	15 knots 87·8	2 19·9	546·4
H.M.S. *Warspite* .	7,390	1,555 tons 108·8	90 tons 59·4	1,200 tons 60·0	81·3	8·6	491 27·0	24·25 ft. 10·2	16 knots 96·7	5 46·4	498·4
H.M.S. *Triumph* .	6,640	1,100 tons 69·6	127 tons 63·5	550 tons 27·5	66·4	8·6	473 28·3	29·1 ft. 3·3	13·5 knots 44·8	4 36·0	348·0
Riachuelo (Brazilian) .	5,700	1,600 tons 112·0	104 tons 62·4	800 tons 40·0	62·7	9·0	250 4·0	19·5 ft. 21·4	16·7 knots 96·5	5 43·5	451·5
H.M.S. *Polyphemus* .	2,640	400 tons 24·0[E]	0	200 tons 10·0	29·0	5·0	131 4·0	20·5 ft. 8·5	17 knots 61·9	5 35·5	177·9
H.M.S. *Inconstant* .	5,780	—	188 tons 79·0	680 tons 34·0	57·8	6·4	614 51·0	25·5 ft. 4·3	16·2 knots 87·0	2 17·2	336·7
Esmeralda (Chilian) .	2,900	—	74 ton 44·0	600 tons 30·0	31·9	5·0	200 11·5	18·5 ft. 12·3	18·25 knots 85·3	·0	220·4
H.M.S. *Canada* . .	2,380	—	40 tons 24·0	170 tons 23·5	26·2	3·6	263 22·3	18·5 ft. 10·2	18 knots 17·8	2 14·0	141·6
H.M.S. *Scout* . .	1,430	—	72 tons 4·3	350 tons 17·5	15·7	6·4	120 8.6	13·0 ft. 8·4	16 knots 32·2	8 48·8	141·8
1st C Torpedo Boat .	50	—	—	10 tons ·0[E]	·55	·4	12 1·0[E]	7·0 ft. ·5	20 E 7·75	3 8·1	18·8

N.B.—Minus quantities, and numbers not expressing *actual*

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$=\alpha+\beta+\gamma.$

$\beta=\frac{\sqrt{D}}{100}(M+N+O+P+R+W)$								Total of $\alpha+\beta$.	$\gamma=U+vV+X-Y.$					Grand Total $\alpha+\beta+\gamma$.
M Manœuvring Power. $M=70-\frac{m}{10}$.	N Sails. $N=\frac{A}{3D^{\frac{2}{3}}}$.	O Conning Station. $O=p\sqrt{i}$.	P Duality of Propellers, &c. Propellers, 20. Engine rooms 5. Boiler-rooms, 5.	R Ram Bow as per type.	W Seaworthiness. $W=\frac{wzr\,(6e+f+2)}{8n}$.	$\frac{\sqrt{D}}{100}$.	Total of β		U Economy of Engines $U=\frac{C(3-u)}{100}$.	V Ships Special Torpedo Boats. co-effs. $v=\begin{cases}10\\5\end{cases}$	X Specialities. Values according to circumstances.	Y Defects. Values according to circumstances.	Total γ.	
20E	9·6	42	30	80	36·0	1·09	237·2	916·2	11·7	10	—	—	21·7	937·9
15E	0	35	30	90	19·0	1·04	196·5	780·8	13·0	10	—	—	23·0	803·8
10E	0	40E	30	80	17·5E	1·024	181·7	896·1	0E	10E	—	—	10·0	906·1
20E	16·7	40E	30	90	38·5E	1·024	240.0	926·0	9·0E	10E	—	—	19·0	945·0
15	17·0	20E	30	100	65·3	·975	231·1	777·5	6·1	0	—	—	6·1	783·6
15E	18·0	47	30	80	30·0	·86	189.2	687·6	13·2	0	—	—	13·2	700·8
22E	21·4	14E	0	100	69·0	·815	184·5	532·5	0	0	—	20	-20	512·5
25E	10·3	36E	30E	80	24·0E	·755	155·0	606.5	12·8	0	—	—	12·8	619·3
20E	0	32	30	100	5·0	·514	96·1	274·0	2·0	0	20	—	22·0	296·0
0	27·0	0	5	30	45·0	·76	81·3	418·0	0	0	—	—	0	418·0
24	10·0E	10	25	15	20·0	·54	56.2	276·6	7·2	0	—	—	7·2	283·8
30E	27·0	0	5	50	20·0	·49	64·7	206·3	5·6	0	—	—	5·6	211·9
32E	16·0E	0	25	30	7·0	·38	41·8	183·6	3·5	0	—	—	3·5	187·1
35E	0	0	0	15	3·0	·07	3·7	22·5	0	0	—	—	0	22·5

values are in red ink. "E" stands for estimated.

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SUPPLEMENTARY LIST OF SHIPS WITH THEIR FIGHTING EFFICIENCY, AS PER TABLE.

NAME OF SHIP AND TONNAGE.		TOTAL α.	TOTAL $\alpha + \beta$.	GRAND TOTAL $\alpha + \beta + \gamma$.
	Tons.			
Lepanto (Italian)	13,850	901·4	1122·7	1142·7
Amiral Baudin (French) ...	11,441	722·6	996·7	1017·5
H.M.S. *Camperdown*	10,000	711·7	912·7	942·6
H.M.S. *Edinburgh*	9,150	538·9	730·1	749·1
H.M.S. *Nelson*	7,630	447·3	638·0	666·0
Sachsen (German)	7,400	450·7	597·0	601·2
Caïman (French)	7,240	480·6	646·3	650·8
H.M.S. *Conqueror*	6,200	388·2	548·2	563·7
H.M.S. *Orion*	4,870	273·8	395·8	395·8
New Belted Cruizer	5,000	386·2	540·2	565·7
H.M.S. *Arethuse*	3,750	243·0	307·2	337·2
H.M.S. *Active*	3,080	174·6	208·6	208·6
H.M.S. *Opal*	2,120	102·4	131·7	131·7
H.M.S. *Caroline*	1,420	81·6	111·0	114·2
H.M.S. *Cormorant*	1,137	55·0	84·2	84·2
H.M.S. *Hecla*	6,200	244·5	284·5	336·5
N.B.—2,000 Tons Coal.				

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DISCUSSION.

Capt. G. H. Noel: As I see that Sir Edward Reed is about to do me the honour of addressing you on this occasion, there are two questions which I should like to ask him, with reference to this subject, if I be allowed to do so. They are both with regard to the position of the armour. (1.) Would you give the *Admiral* type more additional armour than the 150 tons proposed in this Paper; and if so, where would you put it, and for what other weights would you substitute it? (2.) The *Camperdown* of 10,000 tons will have about 2,800 tons of armour: in designing a ship of the same tonnage and weight of armour, how would you propose to distribute that armour?

Sir Edward Reed: My Lord and Gentlemen, in the few remarks which I propose to offer upon this Paper, I shall not contravene the excellent principle laid down by the gallant author, where he says that he has endeavoured to avoid as much as possible controversial points. I think that it would be a poor compliment to him and a poor compliment to the Institution to in any way divert the discussion of this Paper from its substance and from its object. At the same time, as the author has done me the honour to refer to me once or twice in the Paper, I would preface my remarks with an explanation of the two points to which he refers: and, in the first place, I wish to say that, although Captain Noel considers, as, indeed, he did, at the time consider, that in my valuation of the armoured tonnage of certain ships, I did not do them justice in leaving out of the displacement tonnage unarmoured parts, I can only say that I am in no better position to-day to put unarmoured tonnage into the armoured tonnage of ships, and ignore the fact of its being unarmoured than I was on that occasion. I think that anyone who knows how ships are now constructed, and is aware that a very considerable part of the displacement of the ship is without the protection of armour at all, will be of opinion that it is rather a strong measure to call upon me or anybody else to reckon, in the armoured tonnage of the ship, the amount of the displacement which is distinctly deprived of all armoured protection. I cannot see any scientific ground, I cannot see any arithmetical ground, I cannot find any ground at all for doing this; because, if the total displacement of a ship is 10,000 tons, and if from two to four thousand tons of that displacement are entirely devoid of the protection of armour, I cannot understand on what principle you are to include the whole displacement of the ship in the armoured tonnage which she possesses. Another point is this, I am said by Captain Noel to have condemned recent ships on almost every account. Now, my Lord, it so happens that I have condemned recent ships entirely on one account, and that account is simply this—that, in my opinion, which must be taken for what it is worth, they do not possess sufficient armoured protection to be able to stand upright at sea after undergoing that amount of injury which it is most reasonable to expect they will undergo. That is the one objection which I have to the ships, and I take the liberty of mentioning this, because people who think but lightly of the subject are very apt, because I assert very much the view which I hold on that one question, and perhaps assert it too much and too often, to think that I am continually finding fault with ships, and finding all kinds of faults with ships, whereas I never have been a caviller at the designs of my successors in any way at all. I have confined, on almost all occasions, my objection to that one great paramount element of danger as I believe it.

Now, my Lord, coming to Captain Noel's Paper more closely, I think one thing it is fair to say of it, and that is that it has this very great value at least—that it must set any one who reads it and studies it,

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thinking over the whole ground of the elements of the efficiency of a ship of war; and that in itself is no slight advantage. We are very apt indeed to lose sight of important elements of value in a ship of war, and he in this Paper puts an obstacle in the way of our doing so, and attempts an appraisement and appreciation of all the elements of value in such a ship. I think that in doing that he has done a very great service; but at the same time I confess that I am in a very great difficulty in understanding on what the value of such a method as he adopts for estimating the comparative efficiency of ships of war is to be based. Captain Noel at the outset of his Paper rather insisted I think upon the statement which I do not find in the Paper, that in his opinion it was desirable to have a formula that should apply to all ships. In my humble opinion the great defect of this Paper lies in the very fact that it presents formulæ which are regarded as applicable to all ships; because to me it seems that no formula and no principle can possibly justify us in at all comparing certain ships with certain other ships. For instance, Captain Noel has mentioned, and mentioned I think in a very judicious and proper manner, the employment of ships against fortifications; but I should like to know what is the earthly value of a pure ram or a torpedo vessel against a fortification. There can be no sort of comparison whatever for that purpose and for many other purposes between different styles and types of ships; and, my Lord, I think myself that that is a very important consideration. It has always influenced me strongly in this way. I object very much indeed to building ships which can only do one thing if they are to cost so much, when by the expenditure of a little more money upon them they can do that one thing and two or three others besides. That is why I object to any large development of pure rams. I think it will be the greatest possible mistake for this country to develop in large numbers rams or torpedo boats only, excepting that the torpedo boat is a very small craft, and the argument hardly applies to it. But as regards rams, just imagine this – that for every shot which is fired by our ships at sea we fire 100 shots at the shore. If you take the whole range of our action through the periods of peace and war together, for every shot we fire at sea we fire, I believe, at least 100 at the shore. With this fact in view, imagine us investing our money in ships which cannot fire at the shore at all. It seems to me to be a very great mistake—not a mistake to build such a ship as the *Polyphemus*, or a moderate number of such ships, but a mistake to enter upon any large outlay upon any ships which have no armaments, for the reasons which I have stated.

Well, my Lord, I should like to know what is the real value of such comparisons as we have here. I can understand that they would have a value as between ships of an approximately like character. For instance, much has been said by others and by myself—and the matter has been referred to here by Captain Noel—about a remark made by Mr. Barnaby in the *Encyclopædia Britannica* as to displacement being a rough method of valuing the power of ships. I supppose that Mr. Barnaby knows perfectly well, and I know perfectly well, and all of us who use it, and Captain Noel, I think know perfectly well —that it is only valuable as an approximate estimate of the relative value of ships generally like each other. It cannot be of value over any very large range of conditions and circumstances, and I suppose that we may take it for granted that when men like Mr. Barnaby use such a method of comparison they use it as Captain Noel has done, for a general purpose. But then what is the use of massing together and bringing together under a general formula, as is done in this Paper, ships of entirely different types and intended for entirely different purposes. I confess that I cannot understand that. I can thoroughly understand the value of gentlemen applying themselves to the working out of formulæ for comparing ships that are approximately like each other, so as to get a better idea of their relative values than you

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could otherwise do; but I do not myself see the value—it does not at all follow that there is not any value—of a formula which is so comprehensive as this is.

Going a little into detail, I must take exception to one or two points in the Paper. For instance, Captain Noel introduces armour, for example, and he gives it a value according to the total weight carried irrespectively of thickness or position. Now I should like to know if anybody is stupid enough to put two transverse armoured bulkheads very close together in a ship; and people have been unwise enough, in my opinion, to put them a great deal closer than they ought to have been put; but if anybody should put them almost within reach of each other, of what conceivable use would they be? They might be of some use certainly as a transverse bulkhead (I admit that) for preventing shell coming from one side of it to the other, but they would embrace no displacement; they would contribute nothing, or next to nothing, to the effective buoyancy of the ship; they would contribute nothing to the stability of the ship; and it seems to me that to say that armour is to have the same value in a formula, whether it is well-disposed or whether it is ill-disposed, is to blind ourselves to one of the most serious questions, in my opinion, that can possibly affect our judgment with regard to armoured ships.

There is so much in this Paper that I do agree with that I am sorry to have to pick out the points with which I disagree. But I notice that Captain Noel in the Paper does attempt to give value to the reserve of buoyancy protected by armour, but I do not see that he gives any value to the reserve of stability as protected by armour. Now this leads me briefly to advert to a little difference of opinion, perhaps to take a very slight exception to one point in the noble Chairman's speech, because he evidently thought, and a good many think, that the present objection to the *Admiral* class of ships is entirely of the same nature, and only of the same nature, as the objection to the *Inflexible* and other citadel ships. But there exists an entirely novel objection, and to my mind an overwhelming objection, and it is this, that in ships of the *Admiral* class when they are in a state of readiness for sea and with the ends wholly uninjured, they are only capable of being inclined from the upright I think about five degrees, some very small number of degrees, without putting the whole of the armour which they have for their protection under the water; and when the ends are injured, and the water is let into the *Collingwood*, for example, I should not be surprised if you disbelieve me, but it is the fact, the ship is only capable of being inclined 1¾ degrees before all the armour upon her side disappears beneath the sea. What I want to know is this, can a formula be a right formula which takes no account of the fact that in a fighting ship of the first class the circumstance that when a few shells have broken in her side above the armour, whether before the belted part or not, and let the sea in, that that ship no longer retains any armoured stability whatever? Can it be right to leave a consideration like that out of a formula in which you compare the relative values of fighting ships? Now it is said, and will be said, and will be repeated a thousand times, that these ships of which I am at this moment speaking, have all kinds of devices in the form of sub-divisions, and coal spaces, and stores, and things of that kind; and let me make one single remark which sweeps all that matter entirely away, and that is that neither sub-division nor coal, nor stores, nor even cork has any speciality of application to armoured ships. They are just as available for unarmoured ships as for armoured ships; and therefore, when you come to consider the amount of protection which a ship has, or the amount of efficiency she has as derived from her armour, to say that she has some coal or some cork, or to say that she has some stores, has nothing to do with the question, because if she had not an ounce of armour at all she could have the cork and the coal and the stores just the same.

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Another point I have marked is this—it seems very extraordinary, and I protest against it. Captain Noel says: "This measure of coal includes the weight of all the coal carried, or, as it is commonly called, the 'capacity of bunkers.'" Now I object altogether in a formula which has to include speed, draught of water, and a number of things of that kind, to having the ship credited with the ability to carry any amount of coal which you choose to leave space for, and to determine that amount without any reference to her speed, her draft of water, or anything of the kind. I will give the meeting an instance. In the case of the *Imperieuse* and the *Warspite*, I went and looked at the designs of those vessels at the Admiralty, and I found this—that with 7,000 I.H.P. they were to carry a normal coal supply—determined, mark you, by the draught of water and displacement in their sea-going trim—of only 400 tons! But there was a remark upon the legend of the ship stating that she had coal bunker capacity for 900 tons, and when the minister came down to describe these ships to the House of Commons, he took good care to say nothing about the 400 tons, which was the only supply she was to carry at her load draught with the speed she was designed to have, and the displacement and draught of water assigned to her, and other circumstances; but he told the House of Commons that the coal supply was 900 tons. I say that it was not 900 tons any more than the coal supply of any ship in the Service is that for which mere space is provided, without any reference to the weight the ship is able to carry at her designed load draught. I remember a melancholy instance occurring in this connection. When I visited the *Captain*, at Birkenhead, and went round the ship with the late Captain Burgoyne, we passed with a number of officers into the lower deck space where there was a great deal of spare room, which was to be assigned variously for supernumeraries or for coal carriage, and Captain Burgoyne said to me, "Now look what a fine space this is. We can carry by means of this space in this ship 1,100 tons of coal." I turned to him and said, "What can you possibly mean? You know that the ship is already largely over her draught; what can you mean by talking about carrying so large a supply as 1,100 tons of coal when you dare not carry the allowance that is proposed for you, viz., 600 tons, the ship being already largely over her draught;" and he said, "Well, do not say any more about that at present. I want to speak to you about that presently in my cabin." And when I went into his cabin, Captain Burgoyne said, "I am in this position: I have purchased, with the authority of the Admiralty, 800 tons of coal for this ship, but she is very much over draught, and she is very low-sided, and I do not quite like taking her to sea with so much coal as that on board. I would much rather go with a less coal supply." (I think, if I mistake not, he had bought 800 tons of coal, but I am not quite clear on that; but it was either the full supply intended of 600 tons, or the excess supply of 800). I said, "What do you want?" He replied, "I wish—you can do it quite easily by a word at the Admiralty—they would let me transfer 300 or 400 tons of this coal which I have purchased for the ship"—that makes me think it was the 800—"to the next ship which follows," which was the afterwards unfortunate *Vanguard*. I then said to him, "I will do that when I go to the Admiralty;" and I spoke to the gallant admiral whom I see before me about it when I returned, and that was done. But now look what happens. According to Captain Noel he would put 1,100 tons of coal as the efficient supply for the *Captain*. We all know that she could not carry, and did not carry her proper supply; and if she had carried 1,100 tons of coal she would not have lasted a tenth part of the time she did last, but would have capsized long before.

There are a great many other points, my Lord, which invite remark, but I feel that I must already have trespassed beyond the time that is allowed for each speaker, and I will not trespass longer.

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With regard to the questions which Captain Noel put into my hands, I think it will be seen that they are not questions which I ought to attempt to answer under such circumstances. The questions involve the designing of a ship; and though I am always very ready to give the Admiralty all the assistance in my power when they choose to ask for it, I am not ready to design ships, even in outline for them, in this public way. All I can say is this, and that I say in the most emphatic manner—that if I had 2,800 tons of armour to dispose upon a ship of 10,000 tons displacement, the one thing I would not do would be to so dispose that armour as that when she had sustained injury to the unarmoured parts she would put all of it below the water, excepting a strip 130 ft. long and 1 ft. wide.

Admiral Sir Spencer Robinson: My Lord and Gentlemen—Sir Edward Reed has so admirably discussed the Paper, that really, unless in reference to my position as a naval officer, I should scarcely have a single word to say. I, with Sir Edward Reed, offer my sincere congratulations to the gallant officer for the very admirable manner in which he has put before us the requisite qualities for a ship of war, and how in some way or another by certain formulæ, these qualities may enable us to compare one ship with another, but I must reserve most strongly, as Sir Edward Reed has reserved, this one point—that the ships to be compared with each other must be somewhat similar, and, above all, must have been designed for similar purposes. If you design two ships, one for one purpose, and another for a totally different purpose, I do maintain that your table of comparison will not give you the absolute, or even the comparative values of the two ships. I had intended in the short time that is allotted to us to say a word or two about the displacement, but Sir Edward Reed has said it admirably for me.

The total amount of the displacement cannot be, and everybody must know that it cannot be, a fair representative in any way of the value of an armour-clad ship. I have the same remark to make about the weight of armour plating. I should have said a good deal about the disposal of the weight of armour plating, but that has been said so admirably for me, and so perfectly laboured and worked out by my friend, who preceded me in this discussion. There is one other point I made a note of, and on which I will say one word, although I cannot hope to carry conviction to the mind of any man who has listened unconvinced to what Sir Edward Reed has said at various times on the subject, and what he so ably advanced at the Royal Naval Institution. The stability of the *Collingwood* class, I maintain is not sufficiently preserved by the disposition of the armour. As perhaps some of you know, I was present the other day at a lecture given in connection with this subject by some officer of the Admiralty, and I saw distinctly the plans of the *Admiral* class of ships. I saw the plans of the *Ajax* and the *Agamemnon*, and I saw the plans of the *Amiral Duperré*, I mean the plans, drawings, and models of these ships. I felt sure that an officer belonging to the Admiralty would have taken, and did take, the greatest possible care to cause those models and those drawings to be an accurate representation of the facts as far as he could give them. I saw, and I have already told the public through the medium of the *Times*, the French ironclad ship the *Amiral Duperré*, subjected to a certain supposed amount of injury from fire. The lecturer agitated the water in which this model was afloat, and she, in a very short time, very much less time than I am occupying in talking about it, turned over on her side, and remained bottom up. The lecturer, very kindly, in answer to a question that I put to him, said of the model of the *Collingwood* that was then afloat, that if she had been treated with the same amount of destruction with which the *Amiral Duperré* had been treated, she, too, would have infallibly capsized. I am perfectly sure that

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I understood his meaning. He repeated it with very great emphasis. Here we have a large number of ironclad ships, which, treated with a certain amount of destruction, I am not going to say how much, would infallibly capsize. I want to know whether the stability to which the gallant captain has rightly given a high value in these formulæ for expressing the comparative merit of ships of war, has been calculated with reference to these facts. It is undoubtedly true, that with the amount of destruction that to most naval officers seems perfectly possible, seems more than possible, seems even probable, from the use of machine guns, and under various other circumstances, could be inflicted on ships of the *Collingwood* class—ships with an extremely low belt, and with a vast surface at and near the water-line unprotected, except by what I will only call contrivances—a frightful catastrophe might occur, owing to their want of stability. That being the case I think the formulæ which Captain Noel has so ably put before us, and which are in themselves for many purposes of extreme value, do not justify giving ships of the *Collingwood* class so high a factor of merit as he has done.

Mr. J. D'Aguilar Samuda: My Lord—it would be, I think, quite impossible for anybody to attempt, with a Paper of this description, to do justice to it with reference to dissecting the multitude of propositions which it contains, and dealing with them each in the way in which the fixed attention of the writer must have been given to them, in order to have enunciated his views, and therefore it is only with the leading characteristics of his Paper, on an occasion like the present, that one can deal. If I might be permitted I would deal with them only from that point of view.

First, I would say that the attempt to classify the relative fighting value of a navy is most important, and even if some errors may be found to exist in the mode of estimating the various co-efficients, corrections on these heads will doubtless result from a full consideration of the subject, and I, for one, feel that the Institution has much to thank Captain Noel for in the valuable contribution to its annals that his Paper undoubtedly forms. Then generally, with reference to the observations which have been made as to the necessity of vessels of certain classes carrying sails and not disregarding them, I entirely hold with that, and quite approve it. Then on the main point, which has been the subject of observation by the two preceding speakers, I would like to say a word or two. I am obliged to speak in opposition to the view then expressed, that is with reference to the mode in which the value of armour is described irrespective of position. It is described as being valuable in proportion to the total weight carried irrespective of thickness or position. Now, I think that position is the root of everything. I do not like to venture to refer to observations which I may have made perhaps twenty years ago at first, and often subsequently, when I was dealing with this question, because great changes have taken place since that time, but still I believe that no change has taken place which materially modifies that which I have ventured to urge over and over again, both here and in the House of Commons, as being the right way of dealing with whatever amount of armour you are able to carry, and it was exactly the reverse of that which is given here. My view of the matter is this, that as every ship when armoured is armoured for the purpose of standing up as long as it is possible it can stand up when fighting in line, the armour should be so disposed as to give it the greatest chance of standing up for the longest time before it can come to the end of its life. The point that I want to urge strongly on this Meeting, as I have always endeavoured to urge it strongly in the House and elsewhere, although I fear I have never succeeded in making people realise the view to the same extent as it has forced itself on my mind is this, that armour

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of a thinner description spread over the whole of the hull of a ship where it might become vulnerable, is of infinitely greater value than an amount of thicker armour spread over that part called the vitals of the ship, but which is really deficient in the character of vitality, and for this simple reason, that we have had no evidence at this moment that any shell which has ever passed through an armour plate has, as a shell having passed through that armour plate, by reason of the charge which was contained in it, afterwards acted as a shell, and been exploded by the charge contained in it, and therefore really the armour only has to be pierced by shot and not by shell, which is capable of making only such a hole in the vessel as can be plugged, and consequently would not destroy the vitality of the ship; whereas if there were no armour at all, instead of making a hole which might be plugged, you would, as everybody knows, have a hole which might be 10, 12 or 14 feet long, and which therefore would be of a totally different character. I also think that all the arguments used in this Paper as to the extra necessity for protection with light armour by reason of the quick-firing guns which have been introduced of late years, tend in the same direction, and are good, namely, that whatever armour you are able to carry you should carry over the greatest portion of the sides of your ship that is necessary to maintain the vessel afloat. From the main point of view referred to therefore you cannot accept any formulæ which would be less calculated to give a safe result in your ships than that of accepting the quantity of armour carried wholly irrespective of its position as the foundation for a co-efficient of fighting power given to a vessel.

Mr. W. H. White: Captain Noel has, I think, in this Paper, undertaken a task that is an impossibility, and, if he would excuse me for saying so, I think that he has simply shifted the difficulty a little from its ordinary position, by splitting up the consideration of the elements which affect the fighting efficiency of ships into a great number, and dealing with them seriatim. But it is quite obvious, independently of the somewhat formidable algebraical expressions which appear under each head in his tables, that after all *personal opinion* is represented in the co-efficients. I sympathise with Sir Edward Reed's view, that no formula can possibly be framed which would enable us to compare ships of utterly different types. But setting that aside, and taking the ships which are intended, we will suppose, to fight one another in different navies, I think no two people would settle the co-efficients in the same way. That, after all, is the practical difficulty. We come, after all, to the "personal equation" in the matter, and although Captain Noel has dealt with the matter as he claims to deal with it, with great impartiality and with a desire to be fair all round, I do not think that we can accept even Captain Noel as the appraiser of the fighting efficiency of existing ships, and certainly I do not think that he has helped us to formulæ which will enable us to do the difficult thing which has been attempted so often without success. I remember very well when we began to collect and collate information about war ships at the Admiralty many years ago, that we attempted to frame some simple formulæ for "fighting efficiency," and I was asked to make a suggestion, and I did. It had the advantage of much greater simplicity than we see in Captain Noel's method, and it worked out beautifully in the few cases on which I based my co-efficients. I make that admission with perfect frankness. But when we came to compare vessels, some of which were turret ships and others broadside ships, some of which had high guns, *en barbette*, and some of which had guns low down in batteries, it really became impossible to reach anything like a satisfactory result. And I think that the difficulty becomes intensified—in fact, that the whole thing drifts out of the region of practicability, when we pass from single ships to groups of ships. With numbers of ships acting together it is

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quite evident that the power of distributing the fighting force combined with the power of concentration, is a very important thing. A fleet in which the individual ships are perhaps not so strong as some others might in virtue of its greater numbers be for many services of much greater value than the less numerous fleet. At the same time, I personally am extremely thankful to Captain Noel for having set out in detail, as he has done, the elements which must be considered by everybody attempting the design of a ship of war, giving them separate consideration, and stating his opinion as an experienced naval officer of their relative values. All that is material of a very valuable character and an assistance to those engaged in ship designing. I am sure that the Institution must feel very much obliged to Captain Noel for having given us here the results of his labours. I might criticise a great deal in his formulæ, but I do not desire to do that. There is, however, one question which I should like to ask, and that is why one unfortunate ship out of the whole number in the table has a deduction made for "defects." I think it is the *Triumph*. If I might venture a suggestion, it is probably because Captain Noel knows more about that ship than he does of some of the others, and therefore he has made an allowance off. Some of us who know perhaps something of the others might make allowances off them too. Now, my Lord, I do not wish to transfer to this floor a controversy—I will not say a controversy, but a difference of opinion, which, I think, can only be settled in the way which your Lordship was pleased to suggest in your opening remarks. I hope there will be—I personally hope there will soon be—a proper impartial inquiry into the questions which have been so much debated of late with reference to the design of the *Admiral* class. It is well known that I had to do with the designs of those ships, and I am not, therefore, an impartial person, but I want to say one thing more, especially with reference to the remarks made by Sir Spencer Robinson. It is this: in the design of the *Admiral* class there was never the least intention to protect by thick armour the stability of the ship at large angles of inclination, independently of the assistance given to the armoured portions by the unarmoured. Of course, Sir Edward Reed considers that policy a mistaken one; and he has a perfect right to maintain his view, and he will, as we all know, maintain it with energy and force. I only wish to have it understood that we have not changed our view. In the Italian ships, of the *Italia* and *Lepanto* class (to which I am pleased to observe Captain Noel gave the highest figure of fighting efficiency of any ships afloat) there is no side armour at all, and the thick protective decks are used simply to keep shot and shell out of what, despite Mr. Samuda's judgment to the contrary, I like to call the "vitals" of the ship; that is to say, the magazines and the stoke holds where the explosion of a single large shell would mean absolute disaster to the ship; therefore I think they may fairly be called the "vitals" of the ship. The *Italia* class is the one extreme—no side armour, no pretence of protecting even the initial stability by armour. In the *Admiral* class we are midway between the Italian condition and the condition of an earlier time which I think is best represented perhaps by its description as the "Belt and Battery system," where the armour on the midship battery is associated with that on a belt, and where a considerable range of stability is protected by armour. I do not wish to argue that matter here, but I only want to put the case clearly. There never can have been, whatever may have been thought to the contrary, any contention that in the *Admiral* class the armour was so disposed as to protect the stability at large angles of inclination unaided by the unarmoured parts.

Sir Spencer Robinson: We are all aware of that, I think.

Sir Edward Reed: May I say one word in explanation. It is simply this. The announcement

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which Mr. White has just made is the very first time I ever heard it admitted that these ships were designed without stability protected by armour, and I am firmly of the conviction that if that announcement had been made when the first ship was proposed, Parliament never would have sanctioned it.

Mr. White: Just one word in explanation. I think the confusion may have arisen from this circumstance. The armour belt in the *Admiral* class is of sufficient length (it is little under half the length) to protect the stiffness of the ship when the ends are completely water-logged. I think that is really where the confusion has arisen, and I do not think there has been any intentional misrepresentation.

Sir Edward Reed: No, not at all.

Mr. White: The two things are important. That is, that if the ship's ends are water-logged, the armoured portion gives stiffness, but I am careful to say that at large angles of inclination there never has been any assertion or intention to protect the stability by means of armour alone.

Sir John C. Dalrymple Hay: My Lord and Gentlemen, I do not wish to delay the Meeting or prolong the discussion on this most interesting Paper, but like others, I must say I am very grateful to Captain Noel for having given us the consideration of so many points, and for giving us the opportunity of comparing ships together with regard to their value as war vessels. But after all, although I am not prepared to accept the various values he has given to each item, I consider that he has omitted some points which are of still greater value to be considered under the present condition. He has omitted, and he takes notice of it here, the torpedo defence for our ships, and while we are thinking of guns and the means of defending our ships from gun fire, we are omitting to consider the much more destructive instruments, the ram and the torpedo, which are those which will probably destroy all the ships we see paraded on that list. The comparison of ships with regard to their gun power is only a comparison with reference to one-third of the circumstances which are likely to lead to their destruction, and against which we must provide, and I am very sorry that Captain Noel has not, with his great ability, gone into that question which he has alluded to in regard to the torpedo defence which he has omitted from the Paper he has submitted to us to-day. We know that after all the real element of destruction at sea is the mode in which the ship is handled, and the way in which these serious elements of destruction, the ram and the torpedo, can be avoided. As for instance, as Captain Noel would recognise if he had put a specific value on the *Speedy* and the *El Gamo*, and had changed their crews, we should not have had the historical result which Lord Cochrane gave us when with a 10-gun brig he took a 32-gun frigate, and similarly we have now to consider the power of manœuvring, and the power of defence from those more serious elements of danger, the torpedo and the ram, which I apprehend will in future naval actions, as one of them did at Lissa, decide the fate of most naval battles. Captain Noel in making his reply will, perhaps, allude to these points. I think it would be interesting if we were to hear his mind on the subject, and I am sure it would be most valuable to all who have been instructed by him, and I thank the Meeting for allowing me to say these few words on a point which has not hitherto been touched.

Captain Noel: My Lord and Gentlemen, I find greater difficulty in replying verbally, than in reading

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a Paper I have written. Fortunately in my reply I need only touch upon the few items which have been brought into the discussion, necessarily few as the table cannot be discussed in all its details, until it has been studied, and the relative merits and values of each item duly considered. If there is any gentleman here, who would do me the honour of examining this Table, I think he would find the results are more satisfactory than perhaps the tone of the discussion may have led him to infer.

I cannot quite understand the motive for introducing a means of comparison, by which only ships of a similar nature can be compared; I do not see the use of such a comparison; the object of my Paper is to put all ships of war on a similar basis. It is clearly enough acknowledged that these ships may be constructed for different purposes, for instance some may have no guns, such as the *Polyphemus*. The armament of this ship consists of torpedoes and ram, these are taken into consideration, and given value, as her armament, and the *Polyphemus* is brought into a table of this sort to compare her whole fighting value with that of any other war ship. Mr. White says that a comparison of this sort is almost an impossibility, and that every co-efficient is an arbitrary measure. I partly agree with this statement; the Table only gives the values attached by myself to the different items; but I submit that it might be used as a basis to work from, substituting other co-efficients if mine are not approved.

With respect to the armoured tonnage, which has occupied most attention in the discussion, I still maintain that in any comparison of this sort it is quite impossible to treat of armour otherwise than I have treated it. It is unreasonable to suppose that anything so absurd as to place armoured bulkheads across the ship at close intervals, would be tolerated by the authorities or purchasers of a war ship; or even that any disposition of the armour could be decided upon by unqualified persons. Let the constructors have so many tons of armour to put on to a ship of a certain displacement, it is a mere matter of opinion where they would put it. The ablest men we have in this country differ as to the best position for armour, so that our only course is to appoint a certain weight, and leave it to the constructors to decide how that weight is best distributed, feeling confident that there will be merit in any decision such competent judges may arrive at.

As regards the comparison of the armour of some of the ships on my Table, the *Amiral Duperré* has a belt the whole length of the ship (I was not able to get the exact weight of her armour, but I have estimated it at 3,500 tons), the height of this belt above the water-line is only 3 ft. as compared with a height of 2 ft. 6 ins. in our *Admiral* class. There is a doubt in my mind that this height is sufficient for the centre part of the vessel, but on account of the bow wave, there should be a greater height forward, and perhaps aft as well, as the object seems to me to be the protection of the vessel in the parts that are likely to be under water. Any hole above water is a comparatively easy matter to stop, there being no continual flow of water through that hole; but below water you have considerable difficulty in stopping a hole.

I am therefore in favour of an armoured belt of a certain thickness round the whole ship. It may be considered that the 3-in. belt proposed in this Paper for the *Collingwood's* unarmoured ends would be practically useless, but I would point out that such a belt would not only protect the water-line from machine gun fire, but would also resist anything like a spent shot. Such a shot striking the water. and ricochetting would blunder through an unarmoured side, whereas it would be stopped by a 3-in. plate added to the structural strength of the ship's side; and again this small amount of protection would deflect a fairly heavy projectile striking the ship at an angle. For these reasons I believe (although I am no

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authority on such matters) that if the *Collingwood* had this amount of protection, she would be as efficient a vessel as any afloat. Sir Edward Reed apparently overlooks the fact, that in the *Admiral* class the buoyancy is protected by armour before and abaft the Citadel, there is an armoured deck, which is of considerable value, and which I should not take away when I placed my 3 ins. belt along the otherwise unprotected part of the waterline, the armoured deck would resist the effect of heavy projectiles, while the belt would protect the vessel's water-line from the penetration of less powerful shot.

As regards the coal supply. It may be noticed that I have given value to the whole coal capacity. When a ship goes into action, the chances are she will not have her bunkers full, probably she would have about her "complement of coal" on board, but the ability to carry a larger supply is a very great advantage, and I maintain that as a rule such extra supply of coal does not at all decrease the stability of a ship, on the contrary, if it is placed low, it rather adds to her stability.

Sir John Hay asked about the torpedo defence. Some years ago (in 1880), I ventured to submit some plans to the Admiralty. Since then other plans have been submitted; but up to the present I believe none have been definitely adopted, and I have therefore left the matter in abeyance, suggesting that if treated at all it should be treated as a speciality, and placed in the column reserved for specialities. There is no doubt that torpedo defence is infinitely more important to the fleets of this country than to those of other nations, we shall have to defend our ships against the torpedo boats of any nation with whom we may be unfortunately at war, as we must maintain our fleets at sea, whereas it is optional with the enemy to do so.

Gentlemen, I thank you very much for your patience and kind attention, and only regret that my Paper is not likely to be more useful.

The President: Gentlemen, I am sure you will allow me to convey your thanks to Captain Noel for his Paper, and for the very interesting discussion which has ensued upon it.

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THE USE OF WATER-CHAMBERS FOR REDUCING THE ROLLING OF SHIPS AT SEA.

By P. Watts, Esq., Member of Council.

[Read at the Twenty-sixth Session of the Institution of Naval Architects, 25th March, 1885; the Right Hon. the Earl of Ravensworth, President, in the Chair.]

At the meetings of this Institution in 1883, I read a Paper drawing attention to the use of "water-chambers," for reducing the rolling of ships at sea, and giving an account of some experiments and observations which had been made with the *Inflexible*, for the purpose of testing the value of this device, and obtaining information as to the best mode of applying it. The experiments of which I gave an account had been interrupted by the *Inflexible* being required for service at the bombardment of Alexandria, and the results obtained, although interesting and instructive, were of little value for definitely determining the various points at issue. The experiments which had been contemplated were a series of still-water rolling experiments with various conditions of water-chamber, and a series of sea-way rolling experiments with various conditions of water-chamber. It was hoped that it would be found possible to obtain most of the data required from the former of these, but it was unfortunately this series which was interfered with and had to be given up. The sea-way rolling experiments had also been looked forward to as likely to supply valuable data, but these were regarded as important chiefly in view of the natural prejudice in favour of trying the actual eating of the pudding—for satisfying those who would not be disposed to accept theoretical investigations for the behaviour of ships amongst waves, based merely upon still-water rolling experiments.

It was at first intended to pursue the experiments with the *Inflexible* as soon as she was again available for the purpose, but as the sea-way rolling part of the programme was regarded as having been completed satisfactorily, and as it was inconvenient and costly to make the still-water rolling experiments with the ship while she was in the Mediterranean, it was decided to make still-water rolling experiments with some other ships of the *Inflexible* type, and the *Edinburgh* was finally chosen for the purpose. It was also decided

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to supplement these ship experiments with model experiments, as it seemed pretty certain that the general question could be well dealt with by means of models.*

These experiments had commenced when I read my Paper, and I referred to them at the conclusion of the Paper as likely to settle most of the questions in connection with the subject. They have since been completed, and I have obtained the permission of the Admiralty to place the results before the Institution.

The experiments have been conducted by Mr. Froude, and the results to which I shall have to draw your attention are taken from his report. I may say that it has been my duty to co-operate with Mr. Froude in making these experiments, and that he has preferred that I should bring the subject before the Institution.

I should say at once that the model experiments have proved to be of even greater importance than was at first contemplated. As will be seen presently, their validity is established beyond question, by comparison with the ship experiments, and they have the advantage of being inexpensive, of being rapidly made in every variety, and repeated any number of times, and, most important of all, of being extended to large angles of roll, whereas in the ship experiments, it was only possible to obtain a few degrees of inclination on account of the limited space across which it was possible to run men. In fact, the ship experiments could only be made to give a small portion of the information required, and we have had mainly to depend upon the model experiments for data in dealing with the practical question of the efficiency of water-chambers of different dimensions, &c.

It will, perhaps, be convenient, if I again briefly explain the principle upon which a water-chamber acts. A water-chamber consists of a tank across a ship, which, when in use, in order to be most effective, is filled to a certain height with water. The shape of the chamber, depth of water, &c., are arranged so that as the ship rolls, the water moves from side to side in such a manner as to retard the rolling. The water does just the reverse of what is done by the men in the ordinary experiment of rolling a ship in still water. The running of the men is so timed that the moment represented by the transference of their weight from side to side acts with the righting couple during each return roll, and the ship, therefore, acquires a greater angular velocity on each return roll than she would

* Water-chamber experiments with a model were made at the Admiralty in 1876, before the work of fitting the compartments in the *Inflexible* was taken in hand. Quantitative results were not obtained, but the experiments confirmed generally our anticipations as to the value of the device.

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under the action of the righting couple alone. In order to do this, the men have always to run up hill; they should cross the middle line at the end of each roll when the ship is for the moment stationary, and they should remain on each side as long as possible. Whereas the motion of the water in a water-chamber should be just the reverse of this; it should move so that the moment represented by the transference of its weight from side to side acts in the opposite direction to the righting force during each return roll of the ship, so that she acquires a less angular velocity than she otherwise would on each return roll, and, consequently, the extent of the rolling is diminished. The water in crossing the ship should run down hill, and be as much as possible upon the rising side of the ship when it is stationary.

The extinguishing effect of the water-chamber will depend chiefly (1) upon the quantity of water used and the distance through which its centre of gravity can move as the ship rolls, and (2) upon the timing of the motion of the water with reference to the motion of the ship.

With a given quantity of water in the chamber, the moment represented by the transference of the water from side to side depends upon the distance moved through by the centre of gravity of the water; and as this distance cannot be increased beyond a certain point, it follows that the extinguishing effect of the chamber will not increase beyond a certain amount, however great the angle of roll may be. As a matter of fact the angle at which the maximum transference of water occurs as the angle of roll increases, is generally a moderate one, and beyond this the extinguishing effect of the chamber becomes practically constant however much the angle of roll may be increased.

The speed of a wave, such as can travel across a water-chamber, depends upon the depth of the water; and therefore with a water-chamber of given dimensions, and given period of ship, the timing of the water for moderate angles of roll will depend upon the quantity of water carried by the chamber. Hence the question: "What is the most effective quantity of water to be carried by a given water-chamber?" has been one of the most important of this enquiry.

The *Edinburgh* is a ship of 9,200 tons displacement. Her outline is shown Fig. 1 (Plate I.) in which the water-chamber is shaded. The length of the water-chamber, in a fore and aft direction, is 16 ft., and it extends across the ship to the outside plating on either side. Fig. 2 (Plate I.) shows the water-chamber on a large scale.

The ship was incomplete at the time of the experiments and had no stores or

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equipment on board, and it was seen that her period would be considerably less than the period she would have in her sea-going condition. This was objectionable as likely to impair the results as applied to the finished ship. She was therefore *rolled* and her period ascertained, from which her period in her sea-going condition was estimated; and this period, which was 10 seconds, was given to the vessel by placing a quantity of ballast on the upper and superstructure decks. In this condition her displacement was 7,530 tons, and her metacentric height 7·52 ft.

The model water-chamber was $\frac{1}{20 \cdot 5}$ of the full size chamber. It was carried by a frame which could oscillate upon knife-edges, the level of the knife-edges with regard to the chamber, corresponding approximately to the level of the axis of rotation with regard to the water-chamber in the ship. The frame was weighted so as to have the same period of oscillation as the ship; and in the case of experiments made for comparison with the ship experiments, the frame was given a stability corresponding to that of the ship. In order that this latter condition might be satisfied, it was of course necessary that the respective stabilities of the model and ship should be proportional to the fourth power of their respective dimensions. The suspension of the frame carrying the model chamber was almost frictionless, so that the rate of extinction of its oscillations when the chamber was empty was very small, and was due mainly to air resistance. In the experiments made for comparison with the ship experiments, the frame carried a paddle partially immersed in the water of a tank so as to give it a resistance corresponding to that of the ship.

The "Law of Comparison" for the performances of ships and their models enables the rolling resistance of a full sized water-chamber to be estimated from that of its model. You will remember that this law for speed resistance depends upon the fundamental proposition that "the wave features or surface disturbance produced in the water by the motion of ship and model are to scale, when their respective speeds are proportional to the square roots of their respective dimensions." The law applies equally to rolling experiments by substituting period of oscillation for speed. Thus the configuration and motion of the water in the model water-chamber will be precisely the same to scale as that in the full size chamber, if the periods of oscillation of the model and full size chambers are respectively proportioned to the square roots of the respective dimensions of these chambers. The quantity of water in the model and full size chambers will be proportional to the cube of their respective dimensions; and the distance moved through by the centre of gravity of the water in each, will be proportional to their respective dimensions; hence the moment due to the transference of water from side to side in each will be proportional

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to the fourth power of their respective dimensions; and if the stability of the model corresponds to that of the ship it is obvious that the extinction in each case should be the same.

It was intended to make still-water rolling experiments with the ship and model with three different widths of water-chamber, viz., (1) with a chamber of 43 feet mean width, bounded by bulkheads in the ship at A and A[1], Fig. 2; (2) with a chamber of 51½ feet mean width, bounded by bulkheads at B and B[1], Fig. 2; and (3) with the chamber full size, when its mean width would be 67 feet; but only the first two series of these have been made; *first*, because these were quite sufficient to establish the validity of the model experiments; and *second* because it was evident that with the full size chamber, the resistance offered to rolling would be so great, that the angle of roll which it would be possible to obtain with the means available for the purpose, would be too small to be of much practical value.

It was determined to roll the ship with each width of water-chamber in four conditions; one without water, and three with different depths of water in the chamber; the middle depth being that which preliminary experiments with the model chamber had shown to be the most effective for moderate angles of roll. The preliminary experiments proved that the most effective depth of water was that which made the natural period of the water, for that width of chamber, the same as the natural period of the ship. The first series of experiments was made with the narrowest chamber, that bounded by the lines AA[1], Fig. 2, on the 14th September, 1883; the second series with the chamber extended to the lines BB[1], Fig. 2, was made on the 21st September, 1883. The third series was not made for the reasons stated. The capacity of the chamber in the first series of experiments was 140 tons, corresponding to 36·5 lbs. for the model. In the second series of experiments the capacity of the chamber was 180 tons, corresponding to 46·8 lbs. for the model. The capacity of the full size chamber of the ship is 210 tons, corresponding to 54·5 lbs. in the model. The quantities of water in the full size and model chambers in the two series of experiments were as follows:—

	First Series of Experiments with narrow chamber.			Second Series of Experiments with enlarged chamber.		
For ship in tons . .	19·7	43·3	78.8	39·4	78·8	118·2
For model in lbs. . .	5	11	20	10	20	30

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Figs. 3 and 4 (Plate II.) show *declining angle curves** obtained directly from these two series of experiments with the ship, and the corresponding experiments with the models; the drawn curves being those obtained from the ship experiments, and the dotted, those obtained from the model experiments. These curves give the angle of roll reached by the ship and model at each successive single swing. The pairs of curves for the ship and model for the four experiments in each series are respectively marked AA, BB, CC and DD. The AA curves in each set of curves belong to the condition with no water in the chamber, and the CC curves to the condition with the most effective quantity of water in the chamber. The BB curves belong to the condition with less water in the chamber than the most effective quantity, and DD curves to the condition with more water in the chamber than the most effective quantity.

These curves show at a glance the great steadying power possessed by the water-chambers, so far, at least, as still-water rolling is concerned. For instance, referring to Fig. 4, as the number of men employed in running with the chamber empty and with it filled to the most effective depth was the same, we may assume that the moment operating to roll the ship was in each case the same; yet whereas in the former case an angle of more than 5 degrees off the perpendicular was obtained, in the latter case the greatest angle of roll which could be obtained was less than 3 degrees off the perpendicular. Again, whereas in the former case the angle of roll was only reduced from 3 degrees to 2 degrees in six single swings, in the latter case it required only one single swing to produce the same effect.

Figs. 5 and 6 (Plate III.) show the *curves of extinction value* deduced from the curves Figs. 3 and 4. They give the angle of roll lost per swing in terms of angle of roll. They are respectively marked with the same letters as the curves to which they respectively correspond in Figs. 3 and 4.

From Figs. 3 and 4 it will be seen that, with the most effective depth of water in the chamber, and with no water in the chamber, the curves for the ship and model practically coincide with one another, and that there is considerable agreement between the curves in each of the other cases. But the model and ship experiments also agreed in certain other particulars, proving that the same phenomena, depending upon the same critical conditions, were at work in both.

* It has been usual to call these *curves of extinction*, but Mr. Froude has rejected this as liable to lead to confusion, since it more properly applies to curves in which extinction values are plotted as ordinates.

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The most remarkable of these is, that with the most effective depth of water the extinction remains very large, almost down to zero angle of roll, whereas in any other condition, at a certain small angle depending on the depth of water in the chamber, the extinction due to the chamber vanishes, and only that due to the ship remains: *i.e.* the chamber does nothing towards reducing the rolling after this small angle is reached. Fig. 7 (Plate III.) shews copies of the actual rolling diagrams obtained with the partially extended water-chamber in the three conditions under consideration. They are marked B, C and D, and correspond respectively to the dotted curves marked BB, CC, and DD respectively, Fig. 5. They illustrate at once this very curious feature. In the C diagram which was obtained with the most effective depth of water, the oscillations diminish at an uniform rate down to zero; whereas in the B and D diagrams, which were obtained with depths of water respectively less and greater than the depth corresponding to the C diagram, the rate of diminution of the oscillations somewhat abruptly changes at a small angle and becomes practically that due to the rocking frame alone. The effect on the *curves of extinction value* may be seen in Figs. 5 and 6. The curves marked CC in each are, right down to zero, well above the curves marked AA, the former belonging to the ship with the water-chamber filled to the most effective depth, and the latter to the ship without the water-chamber in use; whereas the curves marked BB and DD, representing the other two conditions, come down to the curves marked AA at a point corresponding to from one to two degrees of roll.

The condition with the most effective depth of water, has been styled by Mr. Froude the "wipe-out" condition; because the oscillations in an extinction experiment are completely abolished or cleaned out, so to speak, by the action of the water-chamber, instead of a residue remaining to be extinguished by the natural resistance of the ship.

In the non-extinctive motion the water takes the form of a mere alternating tidal swing from side to side, in which there is very little waste of energy, its centre of gravity crossing the middle line when the ship is upright. There must be very regular and rectilinear motion in all the particles of water, or the phenomenon cannot subsist, and therefore it cannot take place with more than a certain degree of motion of the water dependent upon the depth of the water and dimensions of the chamber; when the requisite kind of motion fails to be perfectly maintained, the water rapidly passes into a breaking wave or "bore," the timing of the motion being such as to give pretty nearly the maximum extinction for the degree of roll. The rapidity with which the change takes place is shown by the rolling diagrams, Fig. 7. The range over which the non-extinctive motion

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is possible is greater the greater the difference between the natural period of the water and the natural period of the ship. If, therefore, we were to gradually fill the chamber and make experiments at intervals, the period of the water being at first considerably greater than that of the ship, this range would at first be considerable, and would then decrease and become zero in the "wipe-out" condition; after which, when the natural period of the water would become less than the natural period of the ship, it would again increase and would continue to increase until the chamber was full.

Another particular in which the model experiments have been verified by the ship experiments, consists of the effect of the motion of the water upon the period of oscillation. The oscillations of the ship are "forced" by the oscillations of the water in the non-extinctive condition. With the shallow-water condition the motion is such that the righting moment due to the inclination is increased, and the period of the ship is therefore quickened; while in the deep-water condition the motion is such that the righting moment is diminished, and therefore the period of the ship is lengthened.

I have here a model water-chamber which Mr. Froude has prepared for the purpose of exhibiting the phenomena of the "wipe-out" and "non-extinctive" conditions, which may be seen at the close of the Meeting.

It will be observed that there are differences between the results of the model and ship experiments in the extreme deep and shallow conditions; but, as in these cases, at the comparatively small angle dealt with, the water was in the process of transition into the non-extinctive motion, an operation which is extremely sensitive to details of form and other circumstances, exact agreement was scarcely to be expected. In order to secure the agreement obtained, it was necessary to reproduce such small features as beams, carlings, &c., in the model, and it was also necessary to start the experiments with the model from the same angle as the angle obtained with the ship.

In the "wipe-out" condition there is almost exact agreement between the ship and model experiment, as likewise there is in the condition with no water in the chamber, and in all important particulars the model and ship experiments agree; hence the validity of the model experiments for giving trustworthy results for the ship may be taken as fully established.

A large number of experiments were made with models for giving information respecting the ship. In these the paddle was dispensed with, and increased stability was

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given to the frame so as to approximate to steady rolling. The experiments were continued up to considerable angles of roll. The results of one series of these experiments is given (Figs. 8, 9 and 10, Plate IV.), which show curves of extinction value for the narrow chamber, the partially extended chamber, and the full size chamber, corresponding in each case to six different depths of water. Each of the curves is marked with the quantity of water to which it corresponds. It will be seen that at the larger angles the extinction is not nearly so sensitive to depth of water as it is at the smaller angles; and usually the maximum extinction is given by rather a larger quantity of water than that corresponding to the "wipe-out" condition. These curves also show the importance of width in a water-chamber: increasing the width of the chamber from 43 to 51½ feet, roughly speaking, doubles its value; and increasing its width from 43 to 67 feet, roughly speaking, trebles its value.

In the *Edinburgh* it was desirable to find room in the water-chamber for a chain locker, if this could be done without interfering materially with the action of the chamber. Experiments showed that by shaping the locker as indicated by the dotted lines (Fig. 2) it would scarcely affect the resistance of the water-chamber, and would therefore be unobjectionable. This form has been adopted.

It is instructive to contrast the extinction due to the water-chamber with that of the hull and bilge keels, and with the hull fitted with additional bilge keels. This is done in Fig. 11, which shows *curves of extinction value* for the natural ship, for the natural ship with full size water-chamber and the best depth of water, for the natural ship with one foot additional width of bilge-keel, and for the natural ship with two feet additional width of bilge-keel. The curves for the bilge-keel resistance are based on the *Greyhound* experiments, and may be taken as approximately true.

These curves show clearly the great resisting effect of water-chambers as compared with that of bilge-keels and surface friction of ship's bottom at comparatively small angles of rolling, while at large angles the bilge-keel and surface friction resistance are the more effective. At 4 degrees the water-chamber resistance has almost reached its maximum, while the resistance due to even the 2 feet additional width of bilge-keel is very small; whereas when rolling to 12 degrees off the perpendicular, the 2 feet bilge-keels offer as much resistance as the water-chamber, and when rolling to 17½ degrees off the perpendicular, the 2 feet bilge-keels offer twice as much resistance as the water-chamber. The reason of this, as already explained is, that while the water-chamber resistance remains practically constant

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after an angle of about 4 degrees is reached, the bilge-keel resistance increases nearly as the square of the velocity with which the keels move through the water.

Mr. Froude classifies the possible varieties of rolling in a sea way as follows:—

(1.) Fairly regular rolling in coperiodic waves.

(2.) Fairly regular rolling in disperiodic waves.

(3.) Irregular rolling in moderately irregular disperiodic waves.

(4.) Irregular rolling in very irregular waves of any kind.

The first of these is the simplest to deal with mathematically; it is also the most important, because it is the kind of rolling which gives the maximum angle of rolling for a given wave slope. It fortunately turns out that this kind of rolling is the most sensitive to resistance, and therefore the most influenced by additional resistance such as that offered by the water-chamber. In regular coperiodic rolling, the resistance offered by the ship, alone limits the angle of rolling, and consequently by sufficiently increasing the resistance, the rolling may be reduced to any extent. But in the other types of rolling, a portion of the wave slope effect is spent in modifying the period of the ship (in types 2 and 3), or increasing the angle of roll (in type 4), and hence resistance affects the angle of roll to only a limited extent.

The mathematical treatment of type (1) may be taken as fairly representing a condition of frequent occurrence; for although this treatment assumes the waves to be perfectly regular, the results are pretty approximately true, even if they be irregular, as long as they are fairly coperiodic. This is not true of the waves in type (2) which require to be pretty uniform or the condition cannot subsist, a very moderate irregularity of wave system will cause the rolling to degenerate into that described as type (3). Type (2) would therefore be comparatively unimportant were it not that it may be discussed, without much error, in the place of type (3). Analysis has shown that the general character of the rolling of type (3) does not differ greatly from that of type (2); that the *mean angle* of rolling of type (3) is approximately equal to the angle of regular disperiodic rolling of type (2), and that the greater the resistance to rolling in type (3) the more nearly will the rolling approach that of type (2). Moreover, the beneficial effect a given addition of resistance would have on rolling of type (3), calculated from rolling of type (2), would be underrated rather than overrated. Hence investigations of type (2) have been taken as fairly representing those also of type (3), and the discussion of the

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more complex type (3) has been avoided. As to type (4), a confused sea is sometimes met with, in which the effect of the general disperiodicity is small compared with that of the entire irregularity of period, or of steepness of waves. In such a sea, while generally the waves will be either comparatively small, or so unfavourably timed with reference to the motion of the ship, as not to produce important rolling, occasionally the ship will be overtaken by groups of a few waves above the average size and timed favourably, which will produce temporary increase of rolling. These groups of waves will have to expend a large portion of their effect in accumulating angle of roll.

Figs. 12 to 17 (Plate V.) show the probable rolling of the ship under the different conditions of sea stated—without the water-chamber in use, and with it in use, and also with additional bilge-keels of one foot and two feet depth. The drawn curves correspond to the natural ship, and to the natural ship with the water-chamber; and the dotted curves to the ship without the water-chamber but with additional bilge-keels of one foot and two feet depth respectively. Figs. 12, 13 and 14 give respectively *steady rolling in coperiodic waves, steady rolling when the ratio of the ship's period to the wave period is equal to* ·9 *or* 1·09, and *steady rolling when the ratio of the ship's period to the wave period is equal to* ·7 *or* 1·23; and Figs. 15, 16 and 17 give respectively *angle accumulated from rest in one single coperiodic wave, angle accumulated from rest in* 2·5 *successive coperiodic waves,* and *angle accumulated from rest in an infinite number of successive coperiodic waves.* Abscissæ in each set of curves, represent the maximum slope of the waves operating upon the ship, and ordinates, the angle of roll of the ship off the perpendicular. It is evident that the curve for the ship with the water-chamber in the last of the latter set, should be the same as the corresponding curve in the first of the former set, the slight difference noticeable is due to the fact that in the former set the most effective depth of water for large angles of roll was taken, whereas in the latter set the most effective depth at small angles, viz., the "wipe-out" depth, was taken. In each of these sets of curves, the differences between the ordinates of the curves, belonging to the natural ship and to the natural ship with water-chamber, corresponding to any wave angle, measures the effect of the water-chamber when the ship is rolling among waves of this slope, under the various circumstances considered. In the same way, the difference between the ordinates of the curves belonging to the natural ship and to the natural ship with deepened bilge-keels, measure the effect of the bilge-keels. Comparing these sets of curves, the great effectiveness of resistance, both of the water-chamber and bilge-keels, as the condition of steady rolling in coperiodic waves is reached, may be seen.

Additional curves are given in Figs. 11 to 17, showing the effect on the rolling of

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the *Edinburgh* of two water-chambers, each of the same dimensions as the one actually fitted. Two water-chambers were originally fitted in the *Inflexible*, but one had to be given up, as the space was much required for other purposes. In the *Edinburgh* also the comparatively small space set apart for use as a water-chamber was the most that could possibly be spared for the purpose.

In this Paper I have not had time to consider how the safety of a ship must limit the extent of the space or spaces set apart as a water-chamber or water-chambers, but it appears that supposing the safety of the ship not endangered, rolling may be reduced by this means to almost any extent. The comparatively small chambers fitted in the *Inflexible* class, do not, of course, touch the question of safety in the least.

DISCUSSION.

The President: The author of the Paper suggested that Mr. Froude, who is present, should now exhibit the model.

Mr. P. Watts: The bilge keels in Fig. 2 are in addition to the existing bilge keels. Mr. Froude suggested that I had not made that clear in the Paper.

Mr. R. E. Froude here exhibited the model, which consisted of a rectangular box representing the chamber, mounted on a pendulum. One side of the box was glazed, the water was whitened by an admixture of clay, and the inside of the box painted black, in order to show the water plainly. The model was made to oscillate with the chamber empty, and also filled to the three different depths of water, giving severally the wipe-out condition and the two non-extinction conditions.

Mr. H. Liggins: My Lord, I venture to address the Meeting on this subject because upwards of twenty years ago the first introduction of bilge keels in this room was brought to the notice of this Institution by myself in connection with the improvement of the rolling of one of the largest passenger steamers of that day. I have had experience in the Atlantic of travelling in a ship that had the bad reputation of being the most inveterate roller afloat, so much so that the records of this Institution will show that the passengers refused if possible to cross in that ship. The result was that the owners of the ship introduced bilge keels on her, which made her as perfect and as comfortable a ship as could well be imagined for the convenience of passengers, but at a loss of speed of, I think, a knot an hour, which was an inconvenience and an expense which they had to face. The subject has often been brought before this Institution, and bilge keels have been largely introduced by Sir Edward Reed, I think, in all Her Majesty's five troop ships, and in the *Bessemer* steamer most certainly. I believe, they are very common in merchant ships. This plan now under our notice is, in my humble judgment, the most perfect means

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which I have ever seen suggested for mitigating that very great evil and that very serious danger to passengers in connection with excessive rolling, and to all those who have to live in ships it is a particularly disagreeable motion, and upsets occasionally the stomachs of even the most hardy seamen; but for passenger ships it is very important that anything that can be suggested should be well tried, and if it should prove effective, as I think this model has clearly proved it to our judgment to be, it ought certainly to be adopted. The only objection to this is one of a second-rate nature, and that is, that in men-of-war, as we have heard this morning, there is no space to appropriate for any other purpose than the purposes of war. But the mercantile marine ships are now so constructed that they should have spare room for the introduction of a water chamber of the dimensions required in 4,000 or 5,000 ton ships. It ought to be done, and the public should be made safe and comfortable, because there is a great deal of danger, particularly in large ships, if excessive rolling takes place, that the ship might be dismasted, and a dismasted ship is a most inconvenient thing to be in in the middle of the Atlantic, and therefore I hail with great satisfaction this most interesting and, what is of more importance, most simple remedy for preventing the rolling of our ships at sea.

Mr. B. Martell: My Lord, I do not think I should have troubled the Meeting with any remarks were it not for the scant justice that was done last year to Mr. Watts in reading a similar Paper to this. I fear at that time he must have felt that Paper was not very fairly criticised, and that there was scarcely justice done to him for the amount of work done in the matter. It is evident from the experiments and explanations we have had from Mr. Froude, as well as Mr. Watts, that a great deal of thought and time have been given to this subject, and no one can doubt that in a ship sufficiently strong, where from a great proportion of breadth to depth she is liable to roll very much, that such a means as this would be very effective in preventing excessive rolling. In Government ships I can easily understand that it is very desirable, because these ships are very broad, and their cargoes cannot be regulated to diminish their rolling, where they have great initial stability. With merchant ships, of course, that can be regulated to a very great extent, because if a ship is unduly broad, and it is found that she is an uneasy ship, the rolling can be diminished by the captain paying attention to the stowage of the cargo, and a ship that has great initial stability may be so loaded as even to be unstable. The necessity of a provision such as this does not, therefore, apply to merchant ships as it does to Government ships, where, in addition to these considerations, a steady platform is required for fighting the guns; I can, consequently, conceive in such a case it may be very useful. But with reference to the remarks of Mr. Liggins, I should be very sorry, as I said on the last occasion, for it to go abroad that a system like this for diminishing the rolling of ships should be applied to merchant ships. I think there would be a great deal of danger involved in that. I might instance a case now of a ship built about two years ago in the North of England; she had a water ballast tank extending to the height of the hold beams, which contained about 400 tons of water. In the event of leakage, in order to prevent the effects of the momentum of that water moving from side to side, there was a middle-line iron bulkhead fitted, which we, in fact, insist on in these deep tanks when the vessels are classed in Lloyd's Register. The tank did leak to a small extent, and it was sufficient to allow the water to move freely, and the consequence was it carried away this middle-line bulkhead, started the top of the tank, and then a very serious leakage took place, and the ship, in fact, was involved in danger. This tank was additionally strengthened, very

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considerably strengthened, and still some little leakage took place about the caulking round the kelson in the lower part, which it was difficult to make perfectly water-tight, or at all events it was not done. A leakage took place again, and the same result followed, until the owners of the ship were compelled to take the top of the tank off and dispense with it altogether for water ballast purposes—an arrangement which, I need not say, admits of great economy, and is almost a necessary purpose for merchant ships engaged in ordinary trades. This ship is now lying in one of the north-east ports, and they are about to try and make her efficient again. What I want to show is that in merchant ships these parts must be made very considerably stronger if ever this plan were to be adopted of allowing free water in tanks. I should be very sorry for it to go abroad that, although we might approve of it for purposes for which it is intended, such as the case of war ships, that it could be adopted for merchant ships in general. At the same time that I am speaking on this point, I may say that for some vessels I almost wish it could be. I regret to say there is a tendency at the present time to increase the proportionate beam of merchant ships to an undue extent. I drew attention to that last year when I was here. You will remember, my Lord, that on one occasion I read a Paper with reference to the comparative narrowness of some merchant ships as compared with their depth, and since that time in some types of ships the breadth has been increasing in proportion to depth, until it is being carried to an undue extent. I gave a little warning last year in this room on that point, and now I regret to say that during this last winter two of these ships of great proportionate beam have been lost. With regard to one, I have had the opportunity of seeing the protest. From that protest I gathered that the excessive violent rolling of the ship was so great that I have no hesitation in saying that that ship, in my opinion, may be said to have "rolled herself to pieces." She was loaded with a general cargo from America, and in a violent storm her rudder chains broke, which, of course, would be the result, to a very great extent, of violent and undue straining motion, caused by excessive stability; and the consequence was that the ship got into the trough of the sea and rolled so tremendously that she began to leak, and eventually they were obliged to forsake her; another steamer came alongside, and with great difficulty rescued most of the crew. These ships I refer to were from four or six feet greater beam than it has been the custom to build ships of that description and size. I have a strong opinion that by increasing the beam, as they are doing in some instances at the present time, quite as much, if not more danger, is being incurred than there was before with the comparative smallness of beam, to which attention was drawn. The only means, I think, we can resort to where merchant vessels have excessive initial stability is by bilge-keels, as we do at present. That, within certain limits, is certainly an effective way of preventing rolling, as Mr. Watts has shown, and that, in connection with a proper regard to the stowage of cargo, is, in my opinion, all that we can do at present as regards merchant ships.

Sir Edward Reed: As I have on more than one occasion rather urged that these experiments should be continued and the results communicated to us, I think it only right to say how very much indebted we are to the Admiralty, and also to Mr. Watts and to Mr. Froude, for putting the Institution in possession of the information which they have. Mr. Watts in his Paper points very properly to the fact that some more work will yet be required from him before this question has been finally disposed of, or before it has been put into a thoroughly available form for adoption by those who are in a position to

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adopt it, because in the last paragraph of the Paper Mr. Watts says: "I have not had time to consider how the safety of a ship must limit the extent of the space, or space set apart for this purpose." Then he goes on to say, "Supposing the safety of the ship not endangered, rolling may be reduced by this means to almost any extent." Therefore we have, on the one hand, the possibility set forth of reducing to almost any extent the rolling by this simple device, and on the other hand we have an indication of the danger that might come from an unwise adoption of the system. Now I do not think that, in the position to which the subject has been thus far brought, the most careful student of this Paper would be quite justified in adopting the system, because I do not think he would know all that he ought to know as to the limitations, but I hope it is not too much to expect, with such able men as Mr. Watts and Mr. Froude in the employment of the Admiralty devoted to the subject, and looking to its great importance in both directions, both on the side of danger and on the side of reduced rolling, that the subject may be developed and put in an available form for the benefit of the Navy and of the mercantile marine.

Mr. W. Denny: My Lord, I hope Mr. Watts and also Mr. Froude, who has assisted him in the production of the experiments for his Paper, will not be disappointed if they find we are unable to criticise and appreciate this Paper so fully as it deserves. From the amount of data, and the nature of the diagrams employed, a considerable and lengthy study of its contents would be required before it could be properly discussed or appreciated. But, even without being able to give such proper appreciation of the Paper, I heartily concur with the previous speakers in praising the work which has now been presented to us, and in hoping for the still greater results in the future which I believe Mr. Watts is justified in expecting. There is one feature in the Paper which must be very pleasing to the members of this Institution, viz., that we are promised a still further application of the results of model experiments of full-sized vessels. The late Mr. Froude practically initiated such model experiments in connection with speed and power, and it must be particularly gratifying to this Institution to find that, in collaboration with Mr. Watts, his son has initiated similiar experiments in rolling. The more we can attack problems in full-sized vessels by means of model experiments, the more are we enabled, as Sir Edward Reed once remarked, cheaply instead of expensively, to make the experiments necessary to determine many important points. I wish to make one remark with regard to what my friend, Mr. Martell, has said: Should these experiments be carried so far as to enable the results deduced from them to be used in merchant steamers, I am not at all afraid of our ability to give effect to them. It is quite possible for us at the present moment to construct chambers which would contain moving water with safety, and it must be remembered, that in the instance quoted by Mr. Martell, the tank to which he referred was not constructed for this purpose. If, as most naval architects expect, the express type of steamer be widely developed in the future, it is very probable that such means of checking rolling as Mr. Watts, with Mr. Froude's collaboration, has brought before us, will be frequently employed. In such steamers as in war ships we would have the condition of fixed weights favourably to the employment of such means. It will therefore be well for this Institution, while taking Mr. Martell's caution to heart, not to let the idea go forth that we are unable in the mercantile marine to give effect to the admirable work of Mr. Watts and his friend Mr. Froude.

Mr. W. H. White: My Lord, when Mr. Watts read his first Paper on this subject, unfortunately I was not present; I came in as he was concluding his remarks on the discussion. But having been

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familiar with the whole of the arrangements in the ships which have come under review in both Papers I would like to say a word or two. The fact was that, in these broad-beam ships of moderate draught with the weights disposed as they were, it was inevitable that there should be very great stiffness and quickness of rolling motion as compared with the ships that had gone before which had more moderate metacentric heights, and much longer periods, and which gained steadiness in consequence of their longer periods during most of the circumstances of their voyaging at sea. The *Inflexible* was the first ship which had these extreme proportions of breadth to draught, and at a very early period of the construction Mr. Watts had attempted, in connection with his work at the Admiralty, some sort of investigation of the extinctive effect of free water in the hold. I went through Mr. Watts' work which was done at that time with the very greatest interest. It was an attempt at an *à priori* investigation of a very difficult subject. It was quite evident from the simple model experiments to which reference is made on page 31 of this Paper, that a great extinctive effect might be hoped for from water put into properly constructed chambers; but I believe I am right in saying that as early as 1874 water-chambers were actually built in the *Inflexible* at Portsmouth. Ultimately, owing to an increase in the armament and other changes, the forward chamber had to be given up, but the one aft was retained and has been extremely serviceable to the ship. When the *Inflexible* went out to the Mediterranean, and Mr. Watts went with her, the observations that were made were eminently successful, so far as their proof of the steadying effect of the water went; but we could not get any exact measure of what that effect might be, or how much of the ship's steadiness was due to her great beam and peculiar form; and this further investigation we have had to-day really does what could scarcely be attempted on the full scale of the ship herself, but which can be fully worked out with the aid of the model. Now, in these vessels—and this is a point which I want to make very clear—we had to deal with large ships with great inertia and comparatively quick periods. We had, therefore, no consideration of possible danger from the use of water-chambers, and while I thoroughly agree with the necessity for proper and due consideration of the subject in applying the arrangement to smaller ships, I would remark that in these large ships with great stiffness this mode of dealing with the question became the only practicable mode. For example, Mr. Watts has shown us in his diagrams, a fact which I think has somewhat escaped observation, viz., that no practicable bilge keels can give anything like the extinctive effect that can be obtained with a small water-chamber. I think we ought to congratulate Mr. Froude and Mr. Watts, because even in the earlier investigation I know Mr. Froude and Mr. Watts were working together, on having so far quantified the steadying effect of a chamber containing the free water as compared with any possible or practicable bilge keel. In almost any merchant ship of the class that Mr. Martell speaks of, no doubt bilge keels would have very great controlling effect; but in these stiff and heavy war ships, although bilge keels are not actually useless, their influence is extremely small. We were faced with the fact that there was no practicable bilge keel that could control the motion; but here has been discovered, and the scientific analysis has now been put on record of a means of steadying any ship. With regard to the diagram which I asked Mr. Watts to explain, I would add a few words. To those who have followed this subject of the behavour of ships at sea, and have made it their study, that diagram has really a very wonderful interest. It is nothing more nor less than an attempt from model experiments to predicate the behaviour of a ship containing free water amongst any assumed waves. Mr. Froude will tell us that his father, whose memory we all revere, began model experiments on rolling as well as resistance many years ago, and carried them very far; and in

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that process of graphic integration which we owe to him, and which is here carried out in an admirable manner, it seems as if we had the power—if we can know what the waves are going to be—of predicting what the behaviour of the ship will be.

Mr. MACFARLANE GRAY: My Lord, I am sure we must all appreciate very highly the Paper we have just heard. I envy Mr. Watts and Mr. Froude the feeling of satisfaction they must have experienced when they completed this investigation, and found their deductions so beautifully verified by experiment. I would like to have more definite information regarding the extent to which modification of the form of the chamber is allowable to economise space. In contracting the water passage in the middle, as in the *Edinburgh*, what is the effect of the air-vessel action of the end chambers. Is it better to leave the ends as air-vessels, as in the *Edinburgh*, or would it be advisable to connect the side chambers at the top by a pipe to prevent the air-vessel action?

Captain FITZGERALD, R.N.: My Lord, I am not a naval architect, only a seaman, but if I might be allowed to speak at this Institution, I certainly must say that I have listened with great interest to this Paper, and also to Mr. Froude's explanations of the model. The first thing that strikes me is, that although the model is clever and interesting, it does not represent the real behaviour of a ship at sea; and I cannot help thinking, with all regard to their clever mathematical calculations, that if they have founded their calculations simply upon a model of that description, they will be apt to mislead us seamen as to the benefit of carrying loose water in a passage. The motion of a ship at sea by no means works on a fixed spindle, as that does, there is a lurching, scending, surging, and all sorts of peculiar wriggling motions, which only men who have spent their lives at sea can properly appreciate. I think, that although those models are very interesting, they cannot found all their calculations upon them without more experiments in the ship itself. Mr. Froude has shown us that that is rather a dangerous instrument, that we could not absolutely come to the conclusion that because certain things occurred there, with a certain depth of water, that we could apply it to any ship without knowing absolutely and accurately what depth of water we were to put in. In one condition it showed us that it absolutely increased the rolling; it was certainly at a very small angle. I mean there would be all sorts of considerations to be brought in; the relative weight which the water bears to the total inertia of the ship, and things of that sort, would have to be delicately worked out, and I doubt extremely if it would be possible to put it in practice and hand it over to us naval officers as a means of doing what is proposed. I am not speaking of the merchant service. The only way of looking at it is to see whether it is a practical thing to put in our hands, and to enable us to produce a steady gun platform before going into action by letting in so much water. I think we have scarcely at the present time arrived at that stage. If I were captain of one of the *Admiral* class to-morrow, I should think twice whether I let water into my water-chamber for steadying my gun platform before I went into action. I think that the enemy would let the water in fast enough for me, perhaps faster than I should like, and I should be very loath to help him, and I think I would take the risk of a little unsteadiness in the gun platform rather than assist him by letting in loose water. I have a certain prejudice against letting loose water into ships. I am aware that the constructors of those ships tell you that they manœuvre as well with loose water as without to the extent of 500 or 600 tons. That is not a subject I want to go into at

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present. I say that it is scarcely right to assume, because we see certain effects with that model, that 500 tons of water on the *Edinburgh's* armour deck would make her a better fighting ship than she is without. I think Mr. Froude alluded to the *Inflexible*. The Chief Constructor went out in her to Gibraltar, and he said the result of the loose water in the experiments there was very satisfactory. I believe it was a little open to doubt from a naval point of view, because I conversed with an officer of the ship the other day, and he said, so far as he could make out, there was no result, it was exceedingly doubtful; but the constructors made up their minds it was satisfactory. It did not upset her, but as to the extinction of rolling it was not satisfactorily proved to the seamen. I believe the naval architects were satisfied with it. I believe, therefore, we should scarcely accept it without more full and extensive proofs than we have seen yet, although I wish them all success, because anything that they can do to produce a steady gun platform will be undoubtedly a very great advantage.

Mr. R. E. Froude: My Lord, in reference to Mr. Macfarlane Gray's question as to the effect of variation of the shape of the water-chamber, and particularly with reference to the effect of rounding of the floor, I may say that we found that a rectangular box of this kind gave practically the same results as the exact model of the *Edinburgh's* chamber, which had a considerable round of floor due to the round up of the deck of the ship. If you were to round it much more it would have more effect, particularly if the water was not very deep. If you rounded it up very much you would have no water at all in the middle when the ship was upright, and in that case you would certainly have a small angle of rolling with very little resistance. You would, practically, have two sloping water-chambers, one on each side, instead of a level one extending from side to side. At very large angles I do not believe the extinction would be substantially affected by the rounding up; but, generally, I believe that in so far as the rounding up affects the extinction, it would do so injuriously. I quite endorse what Captain Fitzgerald has said as to the necessity of the proportions of water-chambers being thoroughly considered before they are fitted in ships. If water-chambers were generally used in merchant ships, for example, or for corvettes or despatch vessels in the Navy, they would have to be of entirely different proportions from those used in the *Edinburgh*. The *Edinburgh* and others of the *Admiral* class, and the *Inflexible* are, as Mr. White has said, ships of exceptionally great beam and exceptionally quick period. The efficiency of water in a balance chamber to check rolling depends on the relation of the period of the water to the period of the ship, and the period of the water depends on the dimensions of the chamber. In order to slow the period of the water you must make it shallower or longer, or both. If, therefore, you diminish the beam you diminish the width of the chamber across the ship, and if you also have the same period, or a longer period, as you would have in the case of despatch vessels and corvettes—in the *Active*, for example, you would probably have about fifteen seconds period instead of the ten seconds of the *Inflexible* or *Edinburgh*—then you must make the water much shallower in order to get the period of the water anything like the period of the ship. I think the result would be that in corvettes or merchant ships you would have to divide the between deck space into three or four by intervening floors, and have about two feet depth of chamber in each. In reference to Mr. Watts' suggestion that water-chambers might be applied more extensively to ships, so as almost to do away with rolling altogether, I should like to point out that there is one very hopeful feature about the idea in comparison with the attempt to do the same thing by putting large bilge keels. In attempting to diminish the rolling largely by means of adding to the bilge

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keels, in proportion as the first additions you make to the bilge keels are effective in reducing the rolling, the effect of the subsequent additions is diminished, because the efficiency of bilge keels largely depends on the angle of rolling. On the other hand, every additional foot of aggregate length of balanced chamber along the ship adds a practically equal amount to the extinction, and takes an equal amount off the rolling. I think that is a very hopeful feature. I did not quite follow what Mr. White said, that bilge-keels would be specially effective to check the rolling in merchant ships.

Mr. White: My Lord, may I explain. I meant to say that these ships are not nearly so large nor so stiff as the war ships. I really meant that their inertia being much less, while their initial stability was not so great, bilge keels would, therefore, be more effective.

Mr. Froude: Yes, I endorse that—at any rate in reference to the difference of period due to difference of inertia—but I rather think the difference of period due to difference of metacentric height does not affect the extinction. Diminishing the metacentric height with given radius of gyration does not affect the extinction, because it diminishes the speed of the rolling motion and thereby diminishes the resistance concurrently with the diminution of the energy which that resistance has to absorb. I rather think, theoretically at least, that this is so, assuming the resistance due to bilge keels to be as the square of the speed.

Mr. White: I think we really mean the same, Mr. Froude.

Mr. Froude: Most probably. However, the point is not of great importance, and I think whether or no the resistance of balanced chambers can be largely used in all classes of ships to check rolling under aggravated conditions of sea, there can be no question that it can be made of enormous advantage to a war ship by giving a really steady gun platform in circumstances, of frequent occurrence, under which she would otherwise roll probably to 8 or 10 degrees, which would be quite enough to seriously affect the gun practice. I think you can see by these diagrams that by putting in some such balanced chamber as exists in the *Edinburgh*, and certainly by putting in two such, you would, under such circumstances, have almost no rolling at all. It therefore seems to me clear that the device is most useful, quite apart from any use that it may prove possible to make of it for merchant or passenger vessels.

Mr. Watts: My Lord, with regard to the remarks made by Mr. Liggins, it has already been pointed out that bilge-keels are not very effective for small and moderate angles of rolling. For instance, as may be seen from Fig. 11, if the *Edinburgh* were rolling to 6 degrees off the perpendicular, even with two feet additional depth of bilge-keels, her resistance to rolling would only be about one-half that of the natural ship with the water-chamber, and less than one-third that of the natural ship with double the length of water-chamber. Mr. Denny has replied to Mr. Martell's remarks, and I need only say that I quite agree with what he has said. I think there can be noquestion but that it is quite easy to make a box sufficiently strong to hold the water of a water-chamber without danger of its bursting; nor is excessive strength required for the purpose. The chamber may be strengthened to any desired extent, without materially lessening its efficiency, by means of vertical transverse bulkheads, which would also divide up the water into comparatively small volumes. The question of stability must be considered for each

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individual ship. There can be no question whatever that the *Edinburgh* is in no danger from the small chambers fitted in her. Neither would she be in any danger if the chamber were twice, or thrice, as large as it is, and we see by the diagrams that such an arrangement would very greatly improve her behaviour in a sea-way. Mr. Martell told us of a ship which rolled so excessively that she "fairly rolled herself to pieces." I think he will agree that if she had been fitted with suitably shaped water-chambers she would have behaved very differently, and would doubtlessly have been afloat at the present time. I think water-chambers might be fitted with advantage in all ordinary ships that are deep rollers, and where it is important to reduce rolling; and I think it would be found that in such cases, they could be introduced, to a sufficient extent, without any danger to the ship. It is not always convenient or even possible, to load a ship in such a way as to reduce rolling to a desirable extent, but if suitable spaces are available they can always be used as water-chambers when it is wished to do so. With regard to Mr. Macfarlane Gray's remark about the possibility of contracting the chamber at the middle line, the chain-locker fitted in the *Edinburgh* appears to be a case in point. It was found almost necessary to put a chain-locker in the water-chamber, as no other space could be found for it, and by experiment it was found that by suitably shaping the locker it would have very little effect on the resistance. Captain Fitzgerald referred to the experiments with the *Inflexible's* water-chamber. I may say that the water-chamber was not very popular among the officers of the *Inflexible*, because the space it occupied could be ill spared for the purpose. Captain Fisher who was not disposed to accept the results of the rolling apparatus without check, appointed a staff of officers to record the angles rolled through by the ship under the various conditions without the water-chamber in use and with it in use. Every angle the apparatus recorded during certain ten-minute intervals was also recorded by these officers, and at the close of the experiments the records obtained in this way were compared with those given by the apparatus. There were but few differences between the records, and the mean angles obtained from each were exactly the same. Thus the results do not rest upon the apparatus records only. I stated the results in the Paper I read on this subject two years ago, and they are printed in the Transactions. They show that the water-chamber on that occasion did reduce the rolling of the ship to a not inconsiderable extent, and we were in the dark to some extent then, and did not know what was the most effective depth of water to fill the chamber to. In any ship fitted with a water-chamber, the Captain should be informed what should be the depth of the water. This is the only fact he would require to know. But if he were to fill the chamber to any other depth, no harm would result, except that he would not get the full advantage of the chamber. The rolling would be always reduced.

The PRESIDENT: I think that both Mr. Watts and Mr. Froude have every reason to be satisfied with the very able discussion which has followed on the reading of the Paper, and I may also say that I think they will find ground for encouragement to continue the valuable experiments which they have been making. I am sure you will allow me to offer your united thanks to those gentlemen for their very instructive Paper and the very interesting discussion which has ensued upon it.

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MECHANICAL METHOD OF MEASURING A VESSEL'S STABILITY.

By John H. Heck, Esq., Member.

[Read at the Twenty-sixth Session of the Institution of Naval Architects, 26th March, 1885; the Right Hon. the Earl of Ravensworth, President, in the Chair.]

At the end of last year, through the courtesy of the Committee of Lloyd's Register, I was able to lay before Mr. William Denny, of Dumbarton, some proposals to find the stability of a vessel at any draught or inclination by the aid of a sectional model and a lever balance.

Mr. Denny having examined the proposals, very generously offered to make a balance and some models in order that the arrangement might be practically tested, suggesting at the same time some points in the direction of improvement.

A balance having been constructed, experiments were made by myself and the members of Mr. Denny's scientific staff on the models of the three following steamers of very different types, designated for the purpose of reference S.S. A——, P.S. B——, S.S. C——.

The S.S. A—— is a three-decked flush steamer with full lines, the P.S. B—— is a paddle steamer of short and shallow dimensions and tolerably sharp, and the S.S. C—— is a steamer having a raised quarter-deck combined with a bridge house.

From the results of these experiments, which are appended to this Paper, it will be seen that the lengths of the righting levers are shown comparatively, those determined by the machine being to the left, and those taken from Mr. Denny's cross curves to the right, the difference between the two being shown in a column headed "Difference."

In the case of the S.S. A——, it will be seen that at none of the draughts and none of the inclinations does the difference amount to $\frac{1}{10}$th of a foot.

In the case of the P.S. B——, at the deep and medium draughts there are no

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differences amounting to $\frac{1}{10}$th of a foot; at the very light draught of 4 feet the maximum and minimum difference is respectively ·157 of a foot and ·003 of a foot.

In the case of the raised quarter-deck steamer C——, the maximum and minimum differences are respectively at the light draught ·163 and ·01 of a foot, at the medium draught ·142 and ·007 of a foot, and at the deep draught ·12 and ·01 of a foot.

It may be added that in these models the sections were rough off the jigger saw, and not accurately chiselled to shape.

Results are also given of some experiments made by myself at Newcastle on smaller models of the two latter steamers, from which it will be seen that in the case of the raised quarter-deck steamer, the difference at all the draughts and angles is under $\frac{1}{10}$th of a foot, while in the case of the P.S. B—— the maximum and minimum difference is respectively only ·12 and ·00 of a foot.

The two last series of experiments were made on the machine and the models that you see, and which were very kindly made by Mr. Hall, the manager of Palmer's Engine Works.

This machine has several improvements, some of which were suggested while I was engaged on the experiments at Dumbarton, the chief one being the capability of the models being tested on both sides of the vertical, thus giving a check test, and practically reducing any error due to unfair setting or imperfect workmanship.

The models tried consisted of a number of rectangular pieces of yellow pine about $\frac{1}{2}$ an inch thick, having a portion sawn out of each of a similar shape and form to that of the vessel, at intervals of from 10 ft. to 16 ft.

The sections were varnished and held together by two end pieces of teak and eight bolts, thus forming what may be called a contracted sectional model of the inside of the vessel. (Fig. 2, Plate VII.)

A piece of ordinary window glass was recessed into the forward teak end in order that the height of the water in the model could be readily seen. (Fig. 2.)

For large and medium-sized vessels the models should be made to a scale of $\frac{1}{4}$ in. = one foot, and for small vessels to a scale of $\frac{1}{2}$ in. = one foot, the sections being $\frac{3}{4}$ in. thick and taken at intervals of from 16 ft. to 20 ft. according to the length of the vessel.

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In order to give an idea of the dimensions of the models required, it may be stated that in a steamer 258 ft. × 35 ft. × 20 ft., having a raised quarter-deck and erections, the model would be about 13½ ins. × 12½ ins. × 10½ ins.

It may be pointed out that if the thicknesses of the sections were made proportional to Simpson's multipliers, more accurate results should be obtained except for the very reduced draughts. This, however, is not necessary, as the trapezoidal rule is practically near enough when taken only in the longitudinal direction, especially if half-sections are employed at the ends, as can readily be done in the models by simply proportioning the thicknesses of the sections.

The machine or balance employed (Fig. 1, Plate VI.) in the experiments consisted of a mahogany frame A A, attached to a steel bar D, having knife edges, and working upon two steel plates as a fulcrum.

At the one end are two scale-pans E E suspended from knife edges, at the other end of the mahogany frame is a table F fixed to a spindle G working in bearings H H, and which is capable of being turned through any angular interval of 15°.

In the machine illustrated the distance from the centre of the scale-pans to the fulcrum is equal to the distance between the fulcrum and the centre of the table or spindle.

To the underside of the table a frame K is fixed, supporting some balance-weights L which can be raised or lowered so as to bring the centre of gravity of the total weight of the model, the table and the balance-weights in the axis of the spindle G, thus practically enabling a constant weight in one of the scale-pans to balance the model when empty of water at all angular positions.

Before any water is put into the model, the model *should* be fixed to the table and placed in the upright position, weights being put into No. 1 scale-pan until the machine is balanced. The model is then turned through 90° and the balance-weights under the table adjusted so that the machine is still balanced. After this the model should be turned through all the angles required, care being taken to note down on a tabular form the weights respectively necessary to keep the machine balanced when the model is upright, and also when inclined on both sides of the vertical at the angles 15°, 30°, 45°, 60°, 75°, and 90°.

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A copy of the tabulated form (marked "Correction") just mentioned, is given in the Appendix.

In order to determine the righting levers for any draught, the model must be placed in the upright position, and the weights proper for that position (as given upon the tabular form headed "Correction") should be put into No. 1 scale-pan. If water is now poured into the model to the height corresponding to any required draught, it will be necessary to put weights into No. 2 scale-pan to keep the machine balanced, such weight being evidently equal to the weight of the water in the model. The weight of the water in the model will then be determined and the water itself will have a similar form to that displaced by the vessel at the corresponding draught.

The following operations will now give the righting lever for any inclination :—

1st. Turn the model to the desired angle.

2nd. Put the weight previously found necessary to balance the model at that particular angle into the No. 1 scale-pan.

3rd. Add or subtract weights from the No. 2 scale-pan so as to balance the machine.

The actual weight then in the No. 2 scale-pan is that necessary to balance the water in the model when inclined at that particular angle, and from the principle of the lever.

Weight in scale-pan to balance water in model × by its distance from fulcrum equals —weight of water in model × by the distance that the centre of gravity of the water is from fulcrum.

The foregoing equality determines the horizontal distance between the centre of buoyancy of the water and the axis of the spindle, consequently if the distance between the centre of the spindle and the centre of gravity of the vessel as marked on the model is known, the righting lever is at once obtained.

A check test can also be quickly obtained by inclining the model to an equal angle on the opposite side of the vertical. The values obtained should practically agree with that previously determined. Again it will be seen that if the model is inclined at an angle to the

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left of the vertical, the centre of gravity of the water travels further away from the fulcrum, consequently weight must be added to keep the machine balanced, while if the model is inclined at an equal angle to the right of the vertical, weight must be taken out, as the centre of gravity of the water is then nearer to the fulcrum, and as the weight to be added in the one case is practically equal to that taken out in the other, any important error can be detected at a glance.

For deeper or lesser draughts it is only necessary to increase or diminish the water in the model.

Generally the two fundamental operations to be done to determine the righting levers are as follows:—

1st. Find the weight necessary to balance the model when empty of water at all the required inclinations.

2nd. Find the weights necessary to balance the model at the same inclinations when filled with water to the height corresponding to the desired draught.

The difference between the two weights × by their distance from the fulcrum and divided by the weight of water in the model gives the distance that the centre of buoyancy is from the fulcrum.

Referring to the results obtained by testing a ¼ in. scale model of the raised quarter deck S.S. C——, it will be noticed that the righting arms given by the machine are shown for both port and starboard inclinations, the column headed "Mean" being obtained by adding the port and starboard values together and dividing by two. This mean it will be noticed is very close to the calculated values.

Appended is a drawing and description of this balance, directions for making the models, and an explanation of the principle and method of using the machine.

Sketches and description are also given of a modification by which outside sectional models could be employed.

The Appendix may perhaps explain any point which has not been clearly described in the Paper.

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S.S. A—— Righting Lever.

Angle.	By Machine.	By Cross Curves.	Difference.	Draught.
Degrees.	Feet.	Feet.	Feet.	
15	·19	·16	·03	5 ft. 9½ in.
30	1·336	1·24	·096	
45	3·31	3·225	·085	
60	4·789	4·82	·031	
75	5·55	5·58	·03	
90	6·396	6·47	·074	
15	1·654	1·69	·036	13 ft. 0 in.
30	2·862	2·775	·087	
45	3·292	3·28	·012	
60	4·216	4·16	·056	
75	5·478	5·525	·047	
90	6·653	6·66	·007	
15	1·598	1·625	·027	18 ft. 1 in.
30	2·734	2·75	·016	
45	3·568	3·61	·042	
60	4·716	4·66	·056	
75	5·88	5·925	·045	
90	6·828	6·85	·022	

Length 285 feet. Beam 35 feet. Depth 26·5 feet. Type—Three Decks, Flush Vessel.
Results of Tests made on Stability Model at Dumbarton, October, 1884.
Model made to a Scale of ½-in. = one foot.

P.S. B—— Righting Lever.

Angle.	By Machine.	By Cross Curves.	Difference.	Draught.
Degrees.	Feet.	Feet.	Feet.	
15	·488	·49	·002	9 ft. 0½ in.
30	·36	·34	·02	
45	·051	·01	·041	
60	·338	·42	·081	
75	·794	·875	·081	
90	1·2	1·29	·09	
15	1·002	1·09	·008	6 ft. 6½ in.
30	1·572	1·6	·028	
45	1·375	1·29	·085	
60	·638	·57	·068	
75	·2545	·345	·09	
90	1·2	1·26	·06	
15	1·513	1·69	·157	4 ft. 0 in.
30	2·622	2·775	·153	
45	2·56	2·65	·09	
60	1·62	1·675	·055	
75	·287	·325	·038	
90	1·133	1·13	·003	

Length 165 feet. Beam 25 feet. Depth 9·5 feet. Type—Light Draught Paddle Steamer.
Result of Tests made on Stability Model at Dumbarton, 30th October, 1884.
Model made to a Scale of ¾ in. = one foot.

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S.S. C—— Righting Levers.

Angle.	By Machine.	By Cross Curves.	Difference.	Draught.
Degrees.	Feet.	Feet.	Feet.	
15	·76	·87	·11	21 ft. 0 in.
30	1·31	1·3	·01	
45	1·436	1.35	·086	
60	1·41	1·3	·12	
75	1·185	1·075	·11	
90	·846	·75	·096	
15	·653	·7	·047	15 ft. 6 in.
30	1·617	1·61	·007	
45	2·459	2·4	·059	
60	2·467	2.325	·142	
75	1·793	1·7	·093	
90	.898	·8	·098	
15	·94	·95	·01	10 ft. 0 in
30	2·00	2·04	·04	
45	0·018	2·89	·128	
60	3·238	3·075	·163	
75	2·506	2·375	·131	
90	1·2	1·15	·05	

Length 275·5 feet. Beam 35·1 feet. Depth 20·1 feet.
Type—Raised Quarter Deck combined with Bridge House.
Results of tests made on stability model at Dumbarton on 5th November, 1884.
Model made to a scale of ½-in. = one foot.

S.S. C—— Righting Levers, in Feet.

Angle.	Port. Machine.	Starboard. Machine.	Mean. Machine.	Curves.	Difference.	Draught.
Degrees.	Feet.	Feet.	Feet.	Feet.	Feet.	
15	1·007	·82	·9135	·962	·048	10 ft. 0 in.
30	2·00	1·966	1·983	2·04	·057	
45	2·998	2·795	2·896	2·88	·016	
60	3·129	3·14	3·134	3·075	·059	
75	2·489	2·333	2·411	2·387	·024	
90	1·179	1.178	1·1785	1·137	·041	
15	·6038	·637	·6204	·7	·079	15 ft. 6 in.
30	1·6287	1·556	1·592	1·61	·018	
45	2·425	2·338	2·381	2·4	·019	
60	2·347	2·342	2·344	2·318	·026	
75	1·75	1·73	1·74	1·7	·04	
90	·827	·781	.804	·8	·004	
15	·74	·844	·792	.863	·071	21 ft. 0 in.
30	1·352	1·34	1·346	1·28	·166	
45	1·383	1·412	1·397	1·337	·06	
60	1·328	1·38	1·354	1·3	·054	
75	1·04	1·16	1·1	1·08	·02	
90	·697	·846	·771	·75	·021	

Model constructed to ½-in. = one foot. Sections taken at about 16·0 feet intervals.
Length 257·5 feet. Beam 35·1 feet. Depth 20·1 feet.
Type—Raised Quarter Deck combined with Bridge House.
The results are given for angles to the left, and also to the right of the vertical.

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P.S. B—— Righting Levers in Feet.

Angle.	Machine.	Curves.	Difference.	Draugh
Degrees.	Feet.	Feet.	Feet.	
15	1·59	1·69	·1	4 ft. 0 in.
30	2·78	2·775	·005	
45	2·7	2·65	·05	
60	1·78	1·675	·105	
75	·357	·325	·032	
90	1·1	1·13	·03	
15	1·09	1·09	·0	6 ft. 6 in.
30	1·7	1·6	·1	
45	1·368	1·29	·078	
60	·6	·57	·03	
75	·289	·345	·056	
90	1·23	1·26	·03	
15	·58	·49	·09	9 ft. 0 in.
30	·42	·34	·08	
45	·074	·01	·064	
60	·365	·42	·055	
75	·83	·875	·045	
90	1·24	1·29	·05	

Length 165 feet. Beam 25 feet. Depth 9·5 feet. Type—Light Draught Paddle Steamer.
Model constructed to ½ in. = one foot. Section taken at 16·2 ft. intervals.

APPENDIX.

Description of the Stability Balance.—*See* Fig. 1, Plate IV.

A A. Mahogany levers rigidly attached to each other and fixed to a steel bar B, working upon the plates CC, as fulcrum.

D. Steel bar fixed at one end of levers A A, having knife edges from which scale-pans E E are suspended.

F. A mahogany table attached to the steel spindle G which works in the bearings H H; this table can be turned round and kept at any angle of 15° interval by means of the wheel M and the pin N.

K. Frame attached to underside of table F., having a slot cut out as shown, in which the balance weights I work; these balance weights can be raised or lowered and kept at any position by means of a thumb screw which is not shown.

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Note.—These balance weights are not actually necessary; they, however, save a good deal of time. Their function is explained in the Paper.

P. A mahogany support to which the steel plates C C are fixed, and upon which the balance rests and oscillates.

O. A beech pin to prevent the frame A A from oscillating when not desired, as is required when fixing model on table or altering inclination of same.

Note.—It will be seen that holes are drilled in the wheel M at angular intervals of 15°.

Fig. I. is an elevation and plan of the machine by which the later results appended to the Paper were obtained.

It will be seen that the model can be turned through 15, 30, 45 — — — — 180° to the left of the vertical, and also through the same angles to the right of the vertical, thus enabling the model to have either an inclination to port or an inclination to starboard; this is to check the results and also to reduce any slight error due to the model not being set or made correctly. Suppose, for instance, the model is turned to 30° to the left of the vertical and the value of the righting lever obtained is 2·8 ft., set the model now to 30° to the right of the vertical, and suppose 2·9 ft. is the value given by the machine, then $\frac{2{\cdot}8+2{\cdot}9}{2} = 2{\cdot}85$ would be the mean value, and this would be found to practically agree with the value got by correct calculation.

Directions for making Inside Sectional Models.

Fig. 2, Plate VII., is an illustration of such a model. As an example, suppose a model is required of the vessel whose longitudinal plan is shown by Fig. 3, Plate VIII.

1st. First divide the longitudinal plan, Fig. 3, into sections at intervals of 16 ft., as 1, 2, 3, — — — —, etc., taking a half interval at each end as $1\frac{1}{2}$ and $10\frac{1}{2}$, and from the body plan make a tracing of the lines of the vessel at those sections.

2nd. Procure some planks of yellow pine which have been passed through the planing machine, having the thickness marked against the sections in Fig. 2 and Fig. 3.

NOTE.—Any uniform thickness would do; $\frac{3}{4}''$ in practice has been found convenient. The variation in the thickness of the end sections is necessary, because half intervals have been taken at those places.

3rd. Cut the planks into the required lengths, drill eight holes in each piece as shown in Fig. 2, and bolt together the sections with two end pieces of teak by eight $\frac{3}{8}''$ bolts, plane the top and bottom up, mark a longitudinal centre line, take the sections apart and with a square draw a centre line *a a*, on each.

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4th. Mark with a gauge on each section a base line $x\,x$, say $1\frac{1}{2}''$ from the bottom edge, lay the tracing of the body plan upon each section in turn, and with a pencil mark off the respective form of the vessel at that section.

5th. Take the wood sections to the jigger or fret saw machine, and saw out the portions within the pencil marks.

Note.—It will be seen that the portion sawn out of the respective sections corresponds with the form of the vessel at those sections.

6th. Varnish the wood sections, put them together, place the two teak pieces at the ends and bolt the whole together, taking care that the longitudinal centre lines are in the same straight line.

Note.—It will be seen from Fig. 3 that the teak end next the bow section has a piece of ordinary window glass recessed into it; this permits the height of the water in the model to be readily ascertained.

7th. Fix on the small brass fittings in Fig. 2, and the model is then ready for testing.

Note.—The model can first be filled with water to ascertain if it is tight.

It will be seen that if the model is entirely filled up with water, the water would really have the same shape as a contracted model of the vessel.

The same brass fittings can be used for any number of models.

If a number of the wood sections be made, bolted and kept in stock, models can be quickly made, as in that case it is only necessary to mark off the sections and saw them out.

The old models can also be utilised for vessels of slightly larger dimensions; the same applies to the teak ends.

When water is in the model the capillary action causes the water next the glass to be slightly heaped up, the true level of the water can, however, be readily determined, for the water that is heaped up above the true level has a very much lighter shade. No difficulty whatever is experienced in determining the actual height of the water in the model.

Explanation of the principles on which the use of the Balance Fig. 1 is based.

A skeleton sketch of the balance is shown in Fig. 4, Plate VIII.; it will be seen that the distance of the scale-pan E from the fulcrum is equal to the distance of the fulcrum from the centre or axis of the table $=a$ and that the model is shown in the upright position.

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Let weights be put into the scale-pan P until the machine is balanced, find also the weights required in P to keep the machine balanced when the model is inclined at the inclinations shown in Figs. 5 and 6, Plate VIII.

Place the model in the upright position, Fig. 4, and put into scale-pan P the weight previously found necessary to balance the machine, fill the model with water to a height corresponding to the desired draught, say W L Fig. 4, and put into the scale-pan E known weights until the machine is again balanced, then evidently the weights in E will be equal to the weight of the water in the model, consequently the latter is determined. Let this = D.

Turn model to any angle to left of vertical, say 45°, as in Fig. 5, and put the weight which was previously found necessary (to balance the model when empty and at 45° to left of vertical) into the scale-pan P.

Then it is clear that the weight D in scale-pan E will not now keep the machine balanced, for the centre of gravity of the water in the model has travelled farther away from the fulcrum, consequently add weight into the scale-pan E until the machine is balanced, then if the total weight in E is called W we shall have

$$Wa = D\,(a + x) \qquad (1)$$

$$\therefore x = \frac{Wa}{D} - a \qquad (2)$$

and x measured on the scale to which the model is made would be the righting lever if the centre of gravity of the vessel was at the axis of the spindle S; for any other position of the centre of gravity, the correction is obvious, for it is only necessary to draw a normal from its actual position, upon the vertical which is at a distance x from centre of spindle S, for instance if G is the centre of gravity G Z would be the righting lever.

For a check test put model at 45° to right of vertical as Fig. 6, and let the weight which was previously found necessary (to balance the machine when model was empty of water and inclined at 45° to right of vertical) be put into the scale-pan P.

It will be then seen that the weight D (= to water in model) will not balance the machine, for the centre of gravity of the water in the model has travelled nearer to the fulcrum, consequently weight must be taken out of the scale-pan E until the machine is balanced; then if the total weight in E is called W′ we should also have—

$$W'a = D\,(a - x) \qquad (3)$$

$$\therefore x = a - \frac{W'a}{D} \qquad (4)$$

and x measured on the scale to which the model is made would also be the righting lever if the centre of gravity of the vessel was in the axis of the spindle S.

If the value of x obtained in this case differs greatly from the value previously found it would show that an error had been made; if, however, there is only a slight difference add the two values together and divide by two, this will give a mean value.

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The check, it will be noticed, can be seen at a glance, as—

W – D should equal D – W′, that is to say, the weight which has to be added when the model is inclined to left of vertical, as in Fig. 4, must be deducted when model has an equal inclination to right of vertical, as Fig. 6.

Again, the mean value can be got from the weights by one calculation, for by adding together equations (2) and (4)

$$\text{we get } x = \frac{(W-W)}{2D}a = \frac{(W-W')}{W+W'}a = \text{mean value} \quad (5)$$

where as before

D = weight of water in model.

W = weight in scale-pan E when model is inclined at angle θ to left of vertical.

W′ = weight in scale-pan E when model is inclined at angle θ to right of vertical.

a = distance between fulcrum and scale-pan E = distance between fulcrum and centre of spindle on table.

In a similar manner the values can be got for any angle and by proportioning the water in the model for any draught.

TABULAR FORM.

CORRECTIONS.

Angles.	For inclinations to right of vertical.	For inclinations to left of vertical.
Upright.	10 lbs. in scale + ·1 lb. × 11″	
15°	10·1 lbs. in scale.	10 lbs. in scale – ·1 lb. × 12″
30°	10·1 lbs. in scale + ·1 lb. × 4″	10 lbs. in scale – ·1 lb. × 14½″
45°	10·1 lbs. in scale + ·1 lb. × 6″	9·9 lbs. in scale.
60°	10·1 lbs. in scale + ·1 lb. × 9″	9·9 lbs. in scale – ·1 lb. × 8″
75°	10·1 lbs. in scale + ·1 lb. × 14″	9·8 lbs. in scale + ·1 lb. × 10″
90°	10·2 lbs. in scale – ·1 lb. × 3″	9·8 lbs. in scale.

The foregoing shows what weight, for this particular model, must be put into the scale-pans to keep the machine balanced when the model is empty of water, and at the respective inclinations; for example, at an inclination of 30° to right of vertical 10·1 lbs. in one of the scale pans, together with ·1 lb. placed 4″ to the right of fulcrum, will just balance the machine.

Again it will be seen that for an inclination of 30″ to left of the vertical 10 lbs. must be placed in No. 1 scale-pan and ·1 lb. must be placed 14½″ to the *left* of the fulcrum.

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Note.—On the upper side of the balance inches and half inches are marked, so that the foregoing can be done without actual measurement.

× ·1 lb. × 4″ indicates that ·1 lb. must be placed to the right of the fulcrum and 4″ from it.
− ·1 lb. × 14½″ indicates that ·1 lb. must be placed to the left of the fulcrum and 14½″ from it.

By thus shifting a small weight as ·1 lb. along the levers as required, the use of very small weights is obviated.

Note.—The compound stability balance, Fig. 7, Plate VIII., and the modification has not been practically tested. It has, however, been given as an indication of a method by which outside sectional models (*see* Fig. 7) could be used to determine the righting levers. By a similar arrangement it is also possible to utilize the ordinary half-plating model (generally made) to determine the righting levers.

Compound Stability Balance.

This arrangement is a combination of two balances (*see* Fig. 7), termed respectively the model balance and the tank balance.

The following is a description (*see* Fig. 7) :—

A A. Mahogany levers fastened together and attached to the steel bar B, this bar has knife edges working upon the plates C, which are fixed to the slides D.

Z. A table secured to the levers O O, which are keyed on the spindle M, the latter working freely in the bearings N N; this table can be turned through any angle up to 90°, and fixed by means of the pin Y and the quadrant P.

L. A steel bar fixed to the levers A A, and having knife edges upon which the scale-pan K is suspended.

D. Slides capable of being raised and lowered up and down the supports E, by the screw F and the wheel G, thus raising or lowering the balance to which the model is attached.

H. Cross-bar on top of the supports D, upon which the wheel G rests.

J. Cross-bar connected to the slides D and attached at the centre to the screw F.

Note.—The table Z is pivoted, so that the model can be turned right round from the position shown in full lines to that shown in dotted lines; this is for check testing.

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The Tank Balance.

Q. Levers fixed to the tank T and fastened to the steel bar R, which works upon knife edges on the plates S, which are fixed to the supports E.

U. Steel bar fixed to the levers Q, having knife edges and supporting the scale-pan V.

Model for the Compound Balance.—(*See* Fig. 7.)

This consists of a number (according to length of vessel) of pieces of yellow pine, fastened together after having been sawn respectively to the external shape of the vessel at intervals of 20 ft.

The sections of the model should be uniform, and about $\frac{3}{4}$ in. thick at the ends; half intervals could of course be introduced.

Directions for Testing Model.

1st. Fill the tank with water to within a few inches from the top, and put weights into the scale-pan V until the tank balance is in equilibrium.

2nd. Secure the model to the table Z, and keep it in the upright position by inserting the pin Y, after which put weights into the scale K until the whole is balanced; if the pin W is then put in, the balance will be prevented from working.

3rd. Lower the model balance until the model is forced into the water, to the depth corresponding to the load draught; take out the pin X and put additional weights into the scale V until the tank balance is in equilibrium; this extra weight added equals the water displaced by the model, and is a function of the vessel's displacement at the load draught.

After this, from the displacement scale, the extra weight necessary in the scale-pan V for any other draught, can be obtained as—

$$\frac{\text{Extra weight in V at load draught}}{\text{Extra weight in V at any lesser draught.}} = \frac{\text{Disp}^{t.}\text{ of ship at load draught}}{\text{Disp}^{t.}\text{ of ship at the same lesser draught.}}$$

As an illustration, the following is given as a description of the procedure for the determination of the righting arms for 90°.

Method of finding Stability Moment at 90° *inclination.*

Turn the table of the model into the 90 position and fix it there, then put weights into K and V until the model and tank balances are balanced; next put in the pin to keep the model balance from working, and the extra weight in V to suit the desired displacement, and force model by the screw F into the water, until the tank balance is balanced.

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2nd. Put in the pin X and take out pin W, the upward pressure of the water will immediately tend to force the model out of the water; if now weights are taken out of K until the model balance is balanced, then evidently weight taken out of $k \times y''' =$ extra weight put into $v \times x'''$ because $y' = y''$.

$$\therefore \text{ if } x'''' = \text{distance of centre of buoyancy from fulcrum,}$$

$$x'''' = \frac{\text{Weight taken out of } k \times y'''}{\text{Extra weight put into } v,}$$

and this gives the vertical line in which the centre of buoyancy acts, consequently, if the centre of gravity of the vessel is known, the righting arm can be determined, and in a similar manner the same can be obtained for any draught or inclination.

Again for a check test, the table Z can be turned around its central pivot so that the model comes to the left hand as shown in dotted lines, when up to certain angles, the righting arms can also be determined, the results, of course, should agree with those previously obtained.

The foregoing expressed algebraically.—

$$G' Z' = x''' - x''.$$

$$\text{where } x''' = \frac{(W' - W'')\, y'''}{(W'''' - W''')}.$$

And

$G' Z' =$ righting arm.
$y' =$ distance centre of tank is from fulcrum.
$y'' = y' =$ distance scale-pan V is from fulcrum.
$y''' =$ distance scale-pan K is from fulcrum.
$W' =$ weight required in K to keep model balance in equilibrium when the model is not in the water.
$W'' =$ weight required in K to keep model balance in equilibrium when model is in water for a particular displacement D.
$W''' =$ the weight required in V to keep the tank balance and water in it in equilibrium before model is in water.
$W'''' =$ the weight required in V to keep the tank balance in equilibrium after the model is forced into water in tank to a displacement D.
$x' =$ the distance centre of gravity of vessel is (when in upright position) from fulcrum.
$x'' =$ the distance centre of gravity of vessel is from fulcrum at any inclination θ.
$x''' =$ the distance the centre of buoyancy of the water displaced is from fulcrum at any angle θ.

Note.—The values of W′ and W″ will of course vary at different angles of inclination, and of W′′′′ according to the water displaced by the model.

The value $(x''' - x'')$ must be measured on the scale to which the model is made, in order to find the righting lever of the actual vessel.

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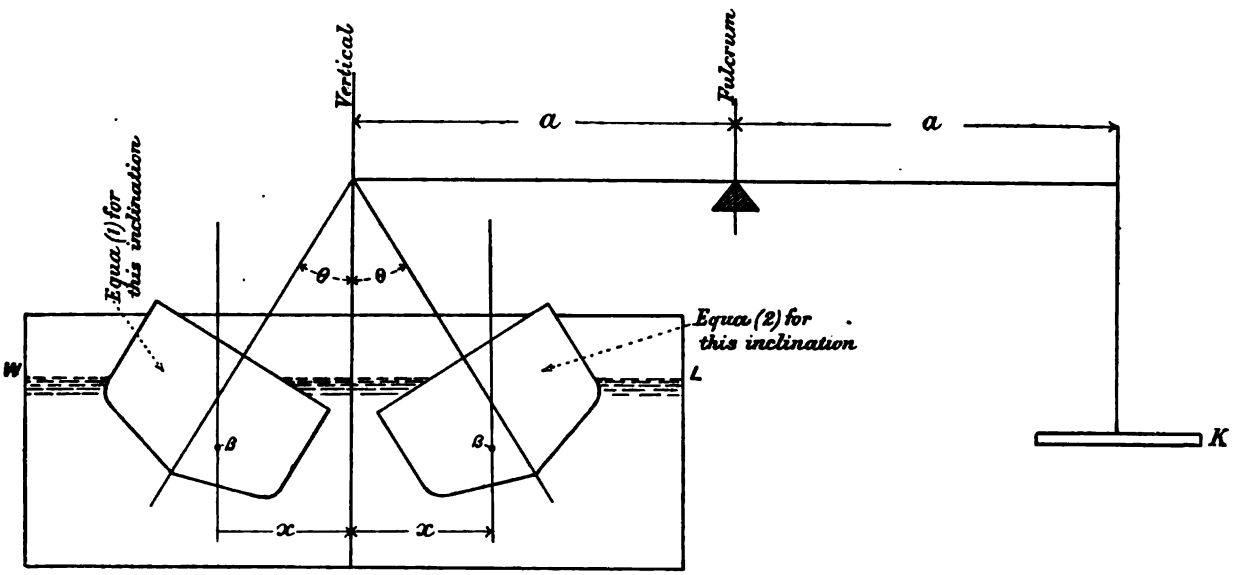
Modification of Plan illustrated by Fig. 7.

In the method illustrated by Fig. 7, the tank is shown attached to a balance in order that the displacement of the model may be measured; this, however, is not necessarily requisite, as the water displaced by the model can be determined direct from the moments if an inclination on either side of the vertical is made for.



Let W = Weight taken out of K to balance upward pressure of water when model is inclined at an angle θ to left of vertical.

$\overline{W}$ = Do. do. do. do. to right of vertical.

D = Displacement of model.

a = Arms of balance (shown equal in length).

x = Distance the centre of buoyancy has moved from the vertical, then evidently—

$$Wa = D\,(a + x) = Da + Dx \qquad (1.)$$

$$\overline{W}a = D\,(a - x) = Da - Dx \qquad (2.)$$

adding equa. (1) and (2) $a\,(W + \overline{W}) = 2\,Da$

$$\therefore\ D = \frac{(W + \overline{W})}{2} \qquad (3.)$$

Also by subtracting equation (2) from (1) and substituting the value of D in terms of W and $\overline{W}$, we get—

$$x = a\,\frac{(W - \overline{W})}{W + \overline{W}} \qquad 4.)$$

The two latter equations ∴ determine the displacement and movement of centre of buoyancy for any angle.

Note.—By this method the ordinary half-plating model could be utilized to determine the righting levers.

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DISCUSSION.

Mr. W. Denny: My Lord, it gives me much satisfaction to speak upon this Paper, because my firm had the pleasure of helping Mr. Heck to perfect his very admirable invention. The experiments which he made in our works give promise of great possibilities in the future. The results were surprisingly near those of our own calculations for cross curves of stability. Mr. Heck has, in his modesty, omitted to mention one point which I will make good for him. He discovered by means of this experimental method an error which had passed unnoticed in the calculation of our cross curves. When Mr. Heck made his experiments in our works, he only inclined his model outwards, as he has shewn you on the diagram, to the left. He now inclines the model both outwards and inwards, and thus obtains a most effectual check on the work. Mr. Heck, I assume in his desire to condense his Paper, has omitted to notice one or two points which must have come under his observation. To work his method accurately, it is not only necessary that the machine should be a mechanically perfect piece of workmanship, but that the wood of which the female model is composed should be perfectly non-absorbing. If the wood absorbs water you will have to account not only for the water in the cavity, but for that absorbed. We have been experimenting upon this point and have arrived at the conclusion that mere varnishing is insufficient to obviate this difficulty. We have found that boiling the wood in paraffin overcomes it, but this is an expensive process. From notes just received from our staff, I am informed that by coating the interior of the model with white-lead, practically as good a result is obtained as by boiling the wood in paraffin. The best proof I can give you of our appreciation of this new method is, that we have adopted it as the check method for all our stability work. I desire to say one or two words upon the relationship of the work done for stability in this country to that done by our able and intelligent neighbours, the French. In noticing the values of English and French work in stability, we can do justice to both nations without in any way depreciating the value of the great work done by either. We know the admirable ingenuity with which the French have investigated every possible theory of stability, and I am sure English naval architects have had pleasure in using the researches of our French friends in the furtherance of their practical work. Owing to the peculiarities of our national life, and to the great publicity which we enjoy in this country in technical matters, there is a large popular and useful force of opinion brought to bear on us, which insists on the immediate utilisation of theory. As a consequence English work in stability has consisted very much in its application to common practical work. Our part has been to devise methods by which we could get these calculations made quickly and cheaply. Mr. Heck's is the highest achievement in this line which we have obtained. Such work is different in kind from the French work, but it is in no way inferior to it in value.

One word in addition and I have done. You, my Lord, have always desired that this Institution should give ample encouragement to every young man coming before it and producing work for its approval or criticism. Remembering this, we cannot do less than congratulate ourselves that there is no want of youth in the Institution, and no want of young men to bring forward fresh ideas and work. The day that this Institution fails in youth and youthful effort, will be the date of its decadence. We have good reason to congratulate ourselves not only on the excellent work done by Mr. Heck, but on the fact that such work has been done by a young man.

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Sir EDWARD REED: My Lord, I thought there might be some valuable remarks made on this Paper. I have very little to say about it myself, except this, that I very fully concur with all Mr. Denny has just so well said. There has been a very marked difference, according to my view of the matter, between the methods of calculating stability pursued in France and in this country. It was my business lately to go over the whole ground of stability contributions to science, and I found nothing that could be more general, and nothing that could be more fascinating, from a geometrical point of view, than the labours of French *savants* on this subject. The work of Dupin, which is well known to a great many in this room, is one of the most fascinating scientific treatises that probably ever was written; and a young Frenchman, Lieut. Guyott, adopting a suggestion of Dupin's, gave a most beautiful investigation of the broadest doctrines of stability, based upon an idea which, as I say, Dupin suggested. When Mr. Heck has obtained a vast number of points of centres of buoyancy, the imagination has to go to work and bring all these centres into a surface, and this has been done by the French in a very remarkable manner. They investigated the surface of buoyancy to a very full extent indeed. Why I mention the labours of this young Frenchman is this, that, having obtained the surface of buoyancy, he imagined a sphere to start from a central point, and to expand within the surface of buoyancy until it touched it at a single point. That point possessed a definite quality with regard to the stability of a vessel. He went on imagining the sphere to enlarge, and successively, the sphere being presumed a moist sphere, to damp portions of the surface of buoyancy, and so he created lakes which left islets upon this surface of buoyancy, and these lakes and these islets were all characteristic of the stability qualities of the ship. Now that is a very abstract consideration; but I thought it a very beautiful one, and it illustrates to my mind both the grasp and the interesting character of the French mode of investigation. But when you come to the English investigators, they have always exhibited more disposition to be purely and mechanically practical than was shown by Dupin, and, in our Meetings last year, by M. Daymard, whom I had the pleasure of seeing here for a moment yesterday, although I am afraid he is not here to-day. All these French gentlemen exhibit an entire command of the geometric aspect of the subject, if I might so say, but in all the English labour you get a most practical disposition displayed. I very much doubt whether, among the whole of the labourers in the science of stability, there has ever been a more practical one than Mr. Heck has proved himself. Mr. Denny has justly mentioned that Mr. Heck's mechanical method of calculating the centre of buoyancy of a ship has actually furnished corrections for the ordinary calculations in two or three instances, and that, in fact, it has been used to discover errors in the ordinary methods of calculations—of course not errors in the system, but errors in the manipulation and working out of the system. Now, one of the beauties of Mr. Heck's arrangement, as I think, is this, that it is as large, as broad in its application as the most elaborate investigation of the French geometricians, and at the same time it is so simple that almost any draughtsman in any firm can employ it for the purpose of calculation. There is one thing, my Lord, I should like to say in connection with this invention—because it is an invention and more—and that is, that it has this very good effect, it will encourage, and very largely encourage, shipbuilders and naval architects to make alternative designs and multiplied investigations from which they are now obliged to shrink, because of the labour involved in the stability calculations. I was thinking the other day of the penalties which we labour under from the want of facilities in completing designs and the calculating of designs rapidly. We have just had a public example on a large scale of this difficulty. Early in December last the Admiralty of the country announced its intention of building

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new ships; at the present moment, late in March, contractors are only now examining the designs and specifications of those vessels, and the reason is, that after the Government of the country had decided to build the ships, they had to design them. Now, if we can by devices of this kind render designs and calculations of designs simple and brief, I do not see any reason why the Admiralty should not at any moment be filled with designs of ships of every kind and character which the service can possibly require, so that we might have the work ready to hand. Again, if you go into a private firm, we all know that the conditions there are such that, as Mr. Denny and other gentlemen have stated often in this Institution, they get but a day or two allowed to consider what sort of ship should be built when they are applied to for a contract for a particular type or kind of vessel. I myself think that ships are often not nearly so good as they ought to be, because there has not been the necessary time to devote to them, and we all know here that stability calculations, and particularly extended stability calculations, taking in various draughts of water and various angles of inclination, have been so troublesome, and have occupied so much time, that it has not been practically possible for people to make alternative designs in abundance, and select from among those alternative designs that which is best for the purpose. Mr. Heck's method seems to me to give us a ready means of calculating the essential elements of stability in the readiest possible manner, and also to any extent. Mr. Denny says this system has been used to correct calculations. I should like to ask Mr. Heck if he can tell us what are the measures of the discrepancies between the results obtained by this instrument and by the calculations; because it seems to me that in itself it is as accurate a method as that of the usual calculation. I do not see myself where errors are likely to enter after what Mr. Denny has said as to the producing a non-absorbing surface. That is, of course, a point of great importance, and if there is a great probability that error does not enter into this, or if the errors which do enter into it are infinitesimal, or too small to take into account, I do not see why this should not be depended upon as a first method of calculation, rather than employed merely as a check. I hope it may be developed to such a point that we shall be able to rely upon it as one means of making the calculations which we desire. I think, my Lord, last year we thought we were present at some of the most wonderful developments of calculations of stability which had ever been made, but this year I think we are in the presence of a more remarkable one than any we saw last year, because, while last year we had in view calculations and investigations in a very highly scientific form, we have now under notice a system of calculating the whole movements of the centre of buoyancy of a ship, the calculations required being simply the arithmetical handling of weights and levers. I do think myself that is a most surprising result to have attained to, and I, for one, most heartily congratulate Mr. Heck on having made so a great a step in the progress of science, and I also most heartily congratulate this Institution on having been favoured with the first public communication upon the subject.

Mr. W. Denny: My Lord, one word of explanation only, and it is with regard to the time required for this work. Last year I had the pleasure of describing here two methods of calculating cross curves of stability. I ought to have mentioned that the time then required to produce a complete set of these curves was a little under a week. We have since managed to reduce the time required for such calculations to under three days, but the member of my staff who has been taking charge of the experiments on Mr. Heck's method, writes me as follows: "As to time, if the work be done by a good draughtsman, assisted by an experienced joiner, using the ordinary wood-working machinery for planing and cutting, a

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complete set of cross curves can be made in twenty hours." This excludes the time required for boiling the wood in paraffin, but includes the drying of the painted model. By improving on the making of the model we are likely to reduce this time to sixteen hours.

Mr. B. Martell: My Lord, after the appreciative remarks which have been made by Mr. Denny and Sir Edward Reed on this invention of Mr. Heck and the elaborate expression of opinion with reference to it, I feel that scarcely anything more is necessary to be said by me, were it not I think it is due to Messrs. Denny to say that the great assistance they gave Mr. Heck when we requested him to go down there, the readiness with which they have complied with that request and their great assistance, call for a public expression of gratitude to that firm from all those who feel interested in such a subject as this. We know the great expense they have gone to in fitting their tank, and no one can tell what an elaborate work it is, and what a most efficient work it is, without seeing it; and it is most gratifying to find that after going to the great expense they have, viewing the great amount of intelligence that has been displayed in effecting it, that they have thrown it open on such occasions as this when new inventions have been brought forward, and that there has not been any exclusiveness on their part, and they lay themselves out *con amore*, to do all they can to perfect any invention which may commend itself to them as of practical usefulness. I think it is due to that firm that we should thus publicly express our thanks and appreciation of such public spiritedness.

There is one other cause of congratulation, from my point of view, and that is that Mr. Heck is a member of the Society that I have the honour of belonging to. Knowing as I do the enormous amount of labour he has bestowed on this invention, and the great amount of intelligence he has displayed, I think it reflects the highest credit on him. I must say that I was very pleased to hear the remarks of Mr. Denny and Sir Edward Reed with reference to the merits of the invention, and your known appreciation, my Lord, of the efforts of those young men who devote themselves to investigations of this kind, and bring them before this Institution. I do not think, my Lord, I need say anything more after the remarks that have been made by the previous speakers.

Mr. F. P. Purvis: My Lord, I have a few remarks to make, chiefly relating to what Mr. Denny has said about the balance which Mr. Heck made for us at Dumbarton. I would speak first about the knife-edges of the balance, when the apparatus was first made; we had some trouble with them. The knife-edge at the centre of the machine was the one we found practically, as well as theoretically, to be the one of the greatest importance. The knife-edge at first was made of mild steel, and we found it necessary to substitute for that mild steel, hard steel, and to have a knife-edge working on glass or agate. By so doing we found that we increased the delicacy of the machine to a very great extent. Another point that seems to me of importance is, that instead of working with a level as Mr. Heck does, he should use a long pointer and have a zero point, and by that pointer determine when the balance is upright. The zero would be checked by the level. It seems to me that the pointer saves time as compared with the level. With regard to the application of white-lead, we have employed it not only over the whole inner surface of the model, but between the sections, the object of the application being to prevent the water getting in between the sections as well as the soaking of the water into the sections. For the possibilities of the balance in the future I have very great hope, and I wish it every success.

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Mr. H. H. West: My Lord, I will not detain the Meeting more than a minute or two. It appears to me in reference to what Sir Edward Reed said just now, that the accuracy of the machine and the results depend absolutely and entirely on the accuracy of the model—that is, on there being a sufficient number of sections, and their being sufficiently accurate in their representation of the actual ship. In the plan which Mr. Heck proposed of working with an external model, it seemed to me that you would get a means of measuring with perfect accuracy the length of the righting arm for any given height of centre of gravity. Then, as a point in connection with the mechanism of the machine which has just been referred to by Mr. Purvis, the matter of the knife-edges, in some pendulum experiments which I have made, I found a point a remarkably sensitive method of hanging, and if Mr. Heck can succeed in getting sufficient rigidity—sufficient hardness in the point, to support the weight he has to carry, I think he will find the points are more easy to deal with than knife-edges.

Mr. W. H. White: My Lord, Admiral Paris has asked me just to explain shortly a practice he has followed for some time in calculations of stability. It really amounts to this: that he has been in the habit of constructing a "mean section" from the different water lines, and then, dealing with this mean section (which of course means preliminary work) to calculate the whole of the qualities of a ship at various angles of inclination, and various water-lines. The Admiral will not speak in English, although he can do so extremely well, and he has asked me just to make that explanation.

Then, my Lord, I should be glad if I may be permitted to say a word about Mr. Heck's proposal. Some time ago Mr. Heck, who is now a neighbour of mine in Newcastle, was good enough to ask me to come and look at this model. I only say what I then said to him, that it seems to me one of the most ingenious and most perfect arrangements that I have ever seen in my life. It is entirely a new departure so far as I am aware, because Mr. Heck really makes the water contained, cut off for him at once by the force of gravity, the exact shape that he wants within the ship. It is as if he had the ship filled up to any height he liked to work to, only he has got the volume to be dealt with very much shortened. Of course it is perfectly true, as Mr. Denny and Sir Edward Reed have said, that Mr. Heck has produced an apparatus which can be used by almost anybody, but to produce the apparatus it is only fair to him to say that he needed a most thorough acquaintance with the whole of the principles of stability, or he never would have imagined the fundamental idea.

Mr. Scott: My Lord, I will offer a suggestion to Mr. Heck, seeing that Mr. Heck has so admirably succeeded in making the practical application of his system of pointing out the stability of a ship—that he should try to carry this scheme a little further, and have it applied to every ship—that he should have in every ship a model, so that it might act automatically as the vessel was rolling, and as the different weights were put into different portions or parts of the vessel. I think in this way he might render a very great service to the mercantile marine which has not been before attempted. This is a difficulty that we find constantly in loading ships with various cargoes, and if Mr. Heck can bring this suggestion of mine to a practical use, which I think he can, from the data he has got before him, it will be of immense service to the mercantile marine.

Mr. J. H. Heck: My Lord, there are a few points upon which I would like to say a word or two. Mr. White refers to the employment of a mean section by Admiral Paris in order to reduce stability

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calculations. I have been also working in that direction myself in order to still further simplify the models required for the stability balance. When the idea of using a balance first suggested itself, a full model of the vessel was employed, this, however was considered too expensive. After some time, by a happy thought, I hit upon the plan of making sectional models and then felt convinced that there would be a practical field for this method of determining the righting arms. As already mentioned, in order to still further simplify the models, I have endeavoured to obtain a mean section; my solution is however not satisfactory, and I think if Admiral Paris would kindly give us the method by which he arrives at and determines the mean section it might save me, and possibly others, a great deal of time. I should be very glad indeed to get anything of the kind.

Mr. White: My Lord, the Admiral says that he will be very happy to give this Institution a description in writing.

*Admiral Paris: In reference to this interesting instrument for shortening the calculations for stability, I think I may say that when it is simply a question of determining the centre of buoyancy, and subsequently the metacentre, at different inclinations, it is possible to abridge the operation greatly by means of a simple arithmetical process, viz., the employment of a mean section; that is to say of a transverse section, traced with the mean of all the ordinates of the fore and after part of each horizontal (or water-line) section. Its centre of figure, measured at all inclinations, will always be the centre of buoyancy. For, what is usually done in these processes? Each horizontal section is calculated by means of these same ordinates, and the sum of their surfaces, multiplied by their respective distances, gives the displacement, and they serve for calculating the centre of buoyancy. Very well, the mean section is one part of the operation made *à priori*, and if the surface is multiplied by the total length, determined in the same manner, the displacement is obtained. For the case of a ship in the upright position there is no advantage, for the two sides are similar, and consequently the centre of buoyancy is in the middle, but it is different when an inclination takes place, for the centre of buoyancy shifts from the middle, and in order to find its lateral position recourse must be had to longitudinal sections. Again, one does not know whether for an assumed draft and for an assigned inclination the correct displacement will be found; and, if there is a difference, it is necessary to begin over again with a new draft. With the mean section the work is reduced to the one-sixteenth, if there are sixteen sections, and to the one-twentieth if there are twenty. This is the only advantage of the method, and I have been led to it in consequence of doing all my calculations myself. It has rendered easy for me the study of the change of position of the centre of buoyancy at different inclinations, according as the vessel has much "tumble home," or straight sides. It has enabled me to see easily how the conditions change with the exterior form of the ship. In the case of a pronounced "tumble home" commencing at the load line, the centre of buoyancy varies very little from one side to the other. It is very much the reverse in straight-sided vessels. I should add that the time-saving method which I have just indicated is of no use for finding the metacentre, the ordinates for which may be taken throughout the whole length, and simply for this reason, that the cube of the mean of different numbers is essentially different from the sum of the cube of the numbers.

* The above remarks by Admiral Paris were not spoken at the Meeting, but were, by the unanimous request of the members present, furnished for publication after the Meeting. [*Ed.*]

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Mr. J. H. Heck: It can readily be seen that if in place of making a sectional model a model of the mean section was used it would shorten the work very considerably. Making the model takes up the time; having the model the righting arms can be quickly obtained. Now, my Lord, in reference to Mr. Denny's remarks, I was much pleased to hear Mr. Martell express so ably what I feel; there is no doubt it would be productive of great good if there were a few more shipbuilders like Mr. Denny. Anybody who goes to that gentleman's establishment with an idea, having something in it, ispretty sure to receive encouragement. I myself am much indebted to him for the assistance he afforded me in working out this method. The practical difficulties which have been spoken of in connection with the models, such as the absorption of water by the model, can be got over by a little experience, and, so far as I have gone, in my opinion it is not the absorption but the adhesion of the water to the model which precludes the use of very small models indeed. I find that if the model is kept filled for a couple of hours with water and then emptied, the absorption is so small that it makes little difference to the results, and I therefore think it is the adhesion of the water to the surface of the wood that is the cause of the little differences. Several gentlemen have suggested the use of mercury in place of water; this would, however, be expensive. There is one thing in connection with the balance that has afforded me pleasure, and that is the ease with which, by the aid of the apparatus, I have been able to make clear the principles upon which stability is based to a number of folks who would not have understood it had I endeavoured to explain it by sketches and description. I have been, in fact, surprised to find how little some of the folks engaged in shipbuilding know of stability. I have frequently been told that no calculations are necessary if the vessel is inclined. My Lord, Sir E. J. Reed wishes for a comparison between the result obtained by the machine and those arrived at by calculation. A number of comparisons are given in the early part of the Paper, but, speaking generally, the righting arms can be got within one-tenth of a foot. In the experiments made at Dumbarton the balance was not so complete as the one I have here. Inclinations could only be made on the one side, while in the later machine angles could be employed on both sides of the vertical; this reduces any error due to unfair setting and gives a check. A lad can measure and obtain the weights, and it can at once be seen if he has made any error in the weights, as in all cases the extra weight to be added when the model is inclined in one direction must be subtracted when the model is at an equal inclination in the other direction.

My Lord and Gentlemen, I am much obliged for the kind way in which you have received my paper and for the interest you have taken in it.

The President: Gentlemen, it gives me unusual pleasure to rise on this occasion to convey your united thanks to Mr. Heck for his interesting Paper, and also for his most beautiful model. It strikes me, merely as an outsider, that the great merit of this machine is its extreme simplicity—that it obviates that which has always been held, I think, by members of this Association, to be a practical difficulty, namely, to call upon captains to enter upon, and to form for themselves intricate calculations of stability, depending of course, as they always must do, on the distribution of cargo, trim, and other considerations. There is a most simple invention, which almost a child can understand when it is explained to him. I also entirely endorse the high expressions of approbation and encouragement, which Mr. Denny and other members have so properly given to our young men for coming forward with these ingenious inventions. I also wish to press upon your minds the value that we derive from association with our foreign members. It has been illustrated in the last few minutes by the readiness with which a gallant admiral agreed at

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once to supply the information required. We Englishmen pride ourselves upon our scientific attainments; but, depend upon it, our association with foreign minds, and our comparisons with foreign ideas, is of immense value to this Institution. I have the greatest possible pleasure, Gentlemen, in thanking Mr. Heck. There is only one question further, which is a small question, but still not an unimportant one. I think that it would be very desirable if we could get an idea of the cost of these models. If they are to be brought into common use on board our merchantmen, that is not an unimportant question to consider. Of course, if it is a secret it will remain so; but it would be very desirable with a view to its practical use, to know what would be about the cost. I am sure, Gentlemen, you will all permit me to return Mr. Heck our very best thanks. I think that he must have been himself gratified by the reception which you gave him this morning; and I now offer him on your behalf, our most cordial thanks for the clear way in which he has laid this subject before us, for he was as clear in his *vivâ voce* explanations, as he was with his pen on paper, and he deserves the fullest commendation, as also does another of our young and very rising members, Mr. Purvis, who is an ornament to the institution at Greenwich, of which he was formerly a student.

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THE STOWAGE OF STEAMSHIPS.

By F. P. Purvis, Esq., Member.

[Read at the Twenty-sixth Session of the Institution of Naval Architects, March 26th, 1885; the Right Hon. the Earl of Ravensworth, President, in the Chair.]

The stowage of ships is a subject about which an immense amount must be known. It would be well if it were made also more frequently a subject of discussion, so that the knowledge possessed by those who are daily engaged practically in the work of stowage, might become of use in a more extensive way. The only valuable book upon it, so far as I know, is the highly interesting one of Mr. Robert White Stevens, who was an Associate of this Institution. Every page of that book is marked by laborious research and utmost painstaking to secure facts valuable alike to shipowner and shipmaster; comprehensive treatment is a characteristic; the sizes and weights of bales and boxes usually available; matters peculiar to individual ports—such as port charges—the care necessary to avoid spontaneous combustion; the preservation of the master from imposition, and many other matters, being made the subject of careful treatment. The principal want of the book, if I may be allowed to criticise a book of so monumental a character, is clearness in the statements of what are really the stowage rates of the materials, with which the author—in other respects—deals so fully and comprehensively.

For this present Paper I have to crave some indulgence, as it belongs to a class not often read before this Institution. Mr. Barnaby read a Paper on the subject in 1867, treating of the bearing of it upon the stability and stiffness of a vessel, and emphasizing a suggestion of the Council of this Institution to test these points by obtaining the vessel's period of rolling in still water; this suggestion having been made in a report of resolutions upon the safety of iron ships. I have not spared labour in the preparation of it, but am mostly indebted to the labours of others, viz., of Mr. C. H. Johnson, the chief designer of Messrs. William Denny & Brothers, and many others in the employment of the firm, and to the assistance of Messrs. Galbraith, Pembroke & Co., and many other firms and gentlemen.

One of the foremost matters in the question of stowage is the complete knowledge of how many cubic feet of space are available for cargo in any given steamer. Information

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about this is always procurable from the builders, and should always be obtained by the owner; but care should be taken by the one to state, and by the other to enquire, what portions are included and what excluded. An enquiry into the practice of twenty firms yields the following analysis, a detailed statement of which will be found in Appendix I., *between pages* 88 *and* 89.

Six firms out of the twenty include the beam spaces in their measurements, taking ordinates for measurement of holds ranging between the top of the ceiling and a height one-third the round of beam down from the top of the beam, this range being marked A (Fig. 8, Plate X.); one of the six uses the planimeter and traces the pointer round the inside of ceiling and lining and the top of the beam.

Ten others exclude the beam spaces, taking ordinates ranging (*see* B, Fig. 8, Plate X.) between the top of the ceiling and a height one-third of round of beam down from the underside of beam at centre line of ship; or (in the case of two out of the eight firms) planimeter areas of the sections, by tracing the pointer round the inside of ceiling and lining and underside of beam. Two of the ten firms do not set off the one-third round of beam down from underside, in fixing the highest ordinate for area, but work simply to the underside of beam at centre line of ship; while one takes the highest ordinate at the underside of beam half way between centre and side, which is equivalent to taking it at one-fourth the round of beam from underside of beam at centre line of ship.

One firm includes rather more than half the beam space by taking the highest ordinate at half the depth of the beam. Another firm includes the spaces between the beams, but makes a deduction for the space occupied by each beam of a quantity measured by the depth of the beam, and a fore and aft measurement equal to the depth of the beam.

Three firms measure both inclusively and exclusively, according as grain, tea, &c., are carried on the one hand, or cargoes that will not be stowed between the beams, on the other.

The foregoing analysis applies to the case of beams placed on every frame, or beams placed on alternate frames. When the hold beams are widely apart, space is invariably taken to top of beam, a deduction for actual space occupied by each beam being made by some firms. In measuring the 'tween decks the beam spaces are again included by those firms which include them in measuring the holds; and excluded by those firms which exclude them in measuring the holds. The other peculiarities of measurement mentioned apply also alike to holds and 'tween decks. When in a 'tween deck space there is a difference of round between the beam above and the beam below, the usual thing is for the firms that

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include the beam spaces to take for the height of 'tween decks the distance between levels one-third of round of beam below the upper side of the lower of the two decks, and one-third the round of beam below the underside at the centre line of the upper of the two decks (this height is marked G, Fig. 8, Plate X.); and for firms that exclude the beam spaces to take the distance between levels one-third of round below upper side of the lower of the two decks and one-third the round below underside of beam at centre line of ship of the upper of the two decks (H, Fig. 8, Plate X.). The furthest departure from correct measurement is the taking from top of deck below to underside or upperside of beam above, ignoring entirely the difference of round; this is done by two firms, while some others take this height as a first measure, but deduct from it half, in one case a quarter, of the difference of the round of beam of the two decks.

Besides differing in the inclusion or exclusion of the beam spaces, the twenty firms differ also in the value of deductions they allow for keelsons, deep plate frames, stringers, masts, pillars and ventilators. The usual deduction for the three former, when the structure is of the plate and angle iron type, is a rectangle bounding the projecting portion of the structure, but in several cases this is not done; when the projecting portion is a bulb plate or the flange of an angle iron, there is in the majority of cases no deduction at all, but some firms allow a triangular piece big enough to cover the projection.

For round structures, sometimes the actual space occupied is alone deducted; at others the area of the space is measured by the square circumscribing the circle. One firm makes a difference between grain and bales, adopting the two methods respectively, and the same firm makes a similar difference in measuring the tunnel off from the gross capacities. Pillars are sometimes allowed for, sometimes not.

In dealing with bunkers, the measurement of the 'tween beam spaces follows in a general way the measurement of the holds and 'tween decks, though there are some curious differences, indicating that while some firms consider it easier between the beams to stow coal than general cargo, others consider it less easy to do so; where the frames of the steamer form part of the bunker, *i. e.*, where they are clear of ceiling, some difference exists as to the allowance to be made for the space between them; the majority of firms neglect this space altogether, a few take one-half of it, and one or two take the whole or nearly the whole.

With regard to that portion of the foregoing data which deals with cargo capacities, the inclusion or exclusion of the space betwe the beams is the matter most open

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to discussion, though the dealing with other portions of the structure of the ship should not be neglected.

Tea, as stowed by the Chinese, wool and other bales which can be compressed during stowage, and boxes of small size, all find their way into the 'tween beam spaces; bags—such as rice and grain—and grain in bulk, may (if they do not always) require the least deductions to be made on account both of beam spaces and projecting portions of the structure, while casks and boxes make much broken stowage everywhere.

It seems, then, desirable to distinguish three cases. 1st. The case of bales and boxes, such as tea, stowed between the beams, but not close around keelsons, masts, &c.

2nd. Bulk and bag cargoes, such as wheat and rice, stowed between beams and close everywhere.

3rd. Bales, large boxes, and timber not adapted to good stowage, stowed neither between beams nor close anywhere.

From what has gone before, it is evident that the mere statement of capacities is not sufficient for the guidance of whoever may have to make use of the statement. With it should be given also an explanation of the parts included, especially mentioning whether or not the beam spaces belong to these parts.

The following Table gives some notion of the importance of the various parts for two three-decked steamers of large size, one having water ballast throughout and the other a single bottom; the total capacities (after deducting space occupied by fresh water tanks, chain lockers, and other parts about which there can be no question) being treated as 100,000.

	Water-ballast Steamer.	Steamer with single-bottom.	
1. Beam spaces . . .	7,794*	10,216	* Main deck beams on every frame and therefore not so deep as for other steamer.
2. Masts	130	97	
3. Pillars	35	50	
4. Keelsons	482	27	
5. Hold side stringers . .	—	44	
6. Ventilators and pump casings	252	257	
Nett (deducting foregoing) .	91,307	89,309	
Total capacities . .	100,000	100,000	

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The deductions for 2, 3 and 6 in the above are in plan taken to be the squares circumscribing the structures, and the deductions for 4 and 5 are measured by the rectangles or triangles bounding the projecting portions of the structures, according as the most prominent portions are flat plates or bulbs.

In the matter of capacities it is not safe to assume that if no explanation has been made by the builder the capacities have necessarily been taken in the way most favourable to the ship. Probably not enough importance has been drawn to the matter to make either the builder or the owner so keen upon it as one would expect; and the builder, in the absence of this keenness, has adopted a method of measurement which he invariably employs, without thinking it necessary to draw the owner's attention to the same.

To the builder who wants to look at everything in the interest of the owner the following dilemma occurs; if he works to the top of the beam he promises the owner more space than can (generally) ever be utilised. If he works to the underside and, besides giving the capacity, investigates also the stability of his steamer, taking for one the loaded condition of the ship, he obtains for this condition a result which is not the worst possible. Unless he makes two statements, he will have either his capacity such as to deceive, or his stability such that it does not represent the worst case.

In view of the considerable extent to which use is made of the space between the beams, the general shipbuilder will not be far wrong in allowing for one half of this, by taking his highest ordinate at a point one-third the round of beam below the half depth of beam at middle line of ship, or—if he uses the planimeter—tracing round a line at the half depth of beam. This applies to the case of a beam coming on each alternate frame, and requires modification when the beams are placed on every frame. If in any case the beam has a flange at the bottom instead of a bulb, some further allowance should also be made on account of this. For projections into the holds and 'tween decks it seems desirable to deduct rectangles bounding the projecting structures if these have flanges outwards, and to deduct triangles bounding the projecting structures if they have bulbs outwards. The general nature of these deductions is shown by the shaded portions of the sketches M, N, O, P, Q (Fig. 8, Plate X.) representing sections of a mast, an I-keelson, a bulb keelson, a side stringer and a deep-plate frame respectively; deductions so made are probably excessive for the second of the cases mentioned, and defective for cases 1 and 3. In view, however, of the very small proportion the deductions for these projections bear to the total capacities, as shown by the Table on the foregoing page, they might—in all but

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very exceptional cases—be allowed for by a factor; any error involved could scarcely be greater than the error to which any method of measurement is liable.

The foregoing applies particularly to cargo spaces. In dealing with bunkers, it is necessary to neglect the 'tween beam spaces as really not available for the stowage of coal, on account partly of practical difficulties, partly of allowance for ventilation; but there is no reason why half of the space between the frames should not be allowed. In reserve spaces, available either for coal or cargo, the difference for the measurement, viz., in the one case excluding the beam spaces, and in the other including one half of them, involves two calculations and statements, but the greater practical accuracy of the results obtained justifies this small addition to the labour of obtaining them.

If a general rule be not established, such as indicated above, for the measurement of the 'tween beam spaces and projections, then the builder should state how he has been taking them. I have had the effect of the former upon the centre of gravity of the cargo calculated for two vessels. The one vessel has a moulded depth of 31 feet, and three tiers of beams 10 inches, 7½ inches and 8 inches deep respectively; the height of centre of cargo space excluding the 'tween beam portion is 19·37, and including the 'tween beam portion 19·78, giving a difference of ·41 foot. Taking a homogeneous cargo filling this steamer and sufficient to bring her to her load draught, and combining it with the weight of the steamer and the weight of coal in bunkers, the joint centre of gravity becomes 18·61 and 18·82 respectively, giving a difference of ·21. If, as suggested, one half of the beam spaces be taken, this difference between so taking them and neglecting them is reduced to a little more than 0·1 foot. The other steamer is a quarter-deck vessel of 20 feet moulded depth, and having beams 6 inches deep, placed in this case on every frame; the height of centre of cargo space is 13·82 and 14·09 according as the 'tween beam space is excluded or included; and height of centre of gravity of ship fully loaded with coal and cargo 14·44 and 14·57 according as the 'tween beam space is excluded or included.

The necessity for clearness as to what is included in capacities is obvious when we consider that the utilisation of the dead weight depends upon the space really available for the stowage of cargo, not on a space only nominally available. From the labours of Mr. Martell and his assistants in fixing the load-line of a steamer, and from the yet incomplete labours of the Load Line Committee, the dead weight of a steamer can now be very clearly determined, and it should be the care of all concerned that the capacity is suited to this dead weight.

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What this capacity should be must depend on the trade for which the steamer is built; I understand, for instance, that in a steamer running to India an average cargo may stow at the rate of 57 cubic feet to 1 ton weight, while in a steamer running to Australia it may stow at the rate of 80 cubic feet to 1 ton weight. The cargo space required is thus 40 per cent. more in the one case than in the other, for the same dead weight to be carried.

To secure a fair statement of the capacity from the builder, it is not unreasonable that the owner should demand a comparison between register tonnage and cargo capacities, giving the values of all the other spaces in the steamer which go to make up the total space measured for tonnage by the Board of Trade. For this purpose the tonnage under deck seems to be the space best adapted for the one side of the account, while a Table such as the following (obtained from a three-decked steamer built by Messrs. William Denny & Bros.) would form the other side of the account.

		Units of 100 cubic feet.
	Cargo capacity, exclusive of beam spaces	1888·8
	Reserve „	70·8
	Coal bunkers	285·6
	Chain lockers	8·4
	F. W. Tanks	18·2
Under deck.	Machinery spaces, including tunnel	587·4
	Store and Mail rooms	113·4
	Passenger space	238·1
	Officers and Engineers	18·1
	Steamer and firemen	71·2
	Ship's servants	27·7
	Pantries, &c.	9·3
	Refrigerator room	43·7
	Total capacities	3380·7

The underdeck tonnage for this steamer is 3,675, giving a discrepancy of 294·3 from the sum just obtained. This discrepancy is due chiefly to the 'tween beam spaces below all the decks (232·7), and the volume of the lower deck (26·6), which are included in the Customs measurements, leaving 35 for the volumes of the peaks, and for differences due to

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the two very dissimilar sets of measurements, made by the shipbuilder and the Board of Trade respectively. One thing it is important to note here, viz., that register tonnage is not at all a good measure of the cargo capacities of a steamer. This may be seen by the foregoing Table. If nett register, instead of tonnage under deck were taken as the basis of comparison, a further element of disagreement would be introduced by the very empirical allowance for coal and machinery space. In some quarters at least this is perfectly well understood; but yet the mistake does occur frequently enough of taking register tonnage to be a measure of the cargo capacities. Stevens, for instance, does not sufficiently discriminate between a sailing ship and a steamer, and speaks somewhat loosely (p. 108 and elsewhere) of register tonnage in this respect. Mr. Clement Mackrow, in his most useful pocket-book, gives an approximate ratio between nett tonnage and cargo capacities, which must be misleading for any but a limited number of almost identically contrived vessels.

In the relations between the shipowner or shipbroker and the merchant, there are, I understand, five main headings under which charges for freights are made. These five headings are as follows:—

1. Dead weight.
2. Measurement.
3. Bulk
4. Dozens } All three referring exclusively to liquids.
5. Gallons

For dead weight, the actual weight of the cargo involved is taken, and the freight charged at so much per ton weight. In this country all goods stowing closer than 40 cubic feet to the ton weight count as dead weight, while in India the limit is 50 cubic feet to the ton weight.

For measurement, units of 40 cubic feet (in this country), and 50 cubic feet (in India), are taken conventionally to be one ton. As it is often more convenient to weigh than to measure, another figure has to be adopted for each article of measurement goods, expressing the actual weight which can be put into a space of 40 cubic feet. This figure takes the form of cwt. per ton of 40 cubic feet. It seems difficult to understand how such an awkward form of measurement can ever have arisen. Cubic feet to the ton is assuredly a more rational measure, and one more likely to lead people into modes of thought and action which will secure a proper relation between dead weight and capacities of a steamer. And if cubic feet per ton of weight be used at all, then it answers every purpose;

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any number of tons weight of a certain article can readily be converted into units of 40 cubic feet of space, or, if preferred, a Table can be prepared giving the freight per ton which must be charged on goods stowing more than 40 cubic feet per ton of weight, in order that the freight per 40 cubic feet may always be the same.

Charge for bulk introduces a new measure, viz., a tun of 252 gallons. Bulk measurements are made for wine, beer, and spirits in casks only, one ton having the following equivalents:—

4 hogsheads 8 quarter casks 6 barrels 24 firkins 12 kilderkins 2 puncheons 2 pipes 2 butts 16 octaves	= 1 freight tun.

Wines and spirits in cases, on the other hand, go by measurement, in tons of 40 cubic feet.

Dozens, *i.e.* of bottled beer and mineral waters, are reckoned to stow so many to the freight ton, as under:—

20 dozen quarts mineral water, in casks = 1 freight ton.
25 dozen quarts beer, in cases = 1 freight ton.

Gallons, *i.e.* of oils and liquid varnish, are reckoned to stow 160 to 1 freight ton.

The following Table gives the stowage rates of several descriptions of cargo. For the justification of these rates, the source of information, and the size of the parcels measured, I must refer to Appendix II. A diagram (Fig. 1, Plate IX.) is also given showing these rates set off graphically as abscissæ of a curve, the ordinates of which are density, number of pounds to the bushel, and number of pounds to the cubic foot, according to which of the three vertical scales is used.

The list of cargoes is, of course, not an exhaustive one. For the most part, I have given only such cargoes as enter into the yearly trade of the country, to the extent of 100,000 tons and upwards. As a matter of interest, I append also (Appendix III.) a Table from the Customs returns of last year, showing the quantity of each item, either exported or imported, with the percentage brought to or taken from some of the most important ports of the United Kingdom.

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Rates of Stowage.

	No. of cubic feet to 1 ton weight.	REMARKS.
Coal, Scotch	44	
„ Welsh	40	
„ Newcastle	44	
Manchester Bales	50	The figure may reach 160.
Pig Iron	9	Stowed with as little wood packing as possible.
Alkali, in casks	47	
Wheat	46	Varies from 40 to 52 or more.
Flour	45	
Maize	46	
Barley	58	
Oats	72	
Cargo Rice in bags	50	Varies from different causes; weight placed in each bag, amount of paddy, &c. Cargo Rice generally contains 20 per cent. of paddy.
Tea	83—120	
Raw Sugar, in baskets	50	
Cotton, American	130	
„ Indian	60	Machine pressed.
„ Egyptian	70—220	
Jute	49—77	The closer being very much pressed by hydraulic power.
Wool, undumped	235	
„ washed and dumped	100	
„ greasy and dumped	84	
Potatoes	50	
Bacon and Ham, in cases	64	
Peas and Beans	43—53	
Beef frozen and packed	90—95	
Beef chilled and hung in quarters	120	
Mutton, New Zealand	105—110	
Mutton, River Plate	115	

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In the foregoing Table the figures for the grain in bulk, have been taken from measurements made with the bushel. No allowance has been made for the compression which goes on in the hold of a steamer, though it is known that this is somewhat considerable. Stevens puts it down at between 14 and 15 per cent. for wheat. Mr. Isaac Roberts, in a Paper of much practical importance to the Royal Society last year, gave a Table from which the following is deduced. The Table is appended to a Board of Trade Report for August, 1884.

	Cubic feet of Grain per Ton weight.		Percentage reduction of space due to compression.
	Loosely filled into measure.	Closely filled into measure.	
Wheat.			
Red Winter	45·7	41·9	8·3
Bombay	45·7	42·2	7·7
California	45·7	42·2	7·7
Walla-walla	48·7	44·4	8·8
Bessarabia	45·7	42·2	7·7
Means	46·3	42·6	8·0
Peas.			
American	44·8	41·5	7·4
Indian Corn.			
White American	51·5	47·7	7·4
Mixed American.	50·9	47·7	6·3
Means	51·2	47·7	6·8
Oats.			
Russian	80·0	67·9	15·1
Beans.			
Egyptian	48·7	44·8	8·0
Barley.			
English	57·4	50·9	11·2

The fact that grain is subject to very considerable compression is thus exemplified; and in view of it the stowage rates given in the previous Table are probably somewhat higher than they are in practice. The lower portion of a grain cargo must get compressed

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by the upper, even in loading; in addition to the compression experienced during a voyage, of which Mr. Martell spoke in his Paper of 1880, and for which provision is made by Act of Parliament.

In the Table of stowage rates no attempt has been made, for any of the substances, to allow for broken stowage, the figures being obtained from measurements of parcels where lost space was little or nothing. In Appendix II. mention is made of this lost space for the two substances, cotton and wool. In discussing the methods of measuring I have indicated what portions ought not to be measured as part of the available capacity of a ship; the remainder, then, gives what should be sought to be filled, with little or no further allowance for such an item as broken stowage. That this is not too sanguine, in many cases, is illustrated by the following account of the space occupied by goods in one of Messrs. Galbraith, Pembroke and Co.'s steamers, with which account they have kindly furnished me. It shows more space occupied in the ship than is given by the builders. Several other accounts have kindly been supplied me by the same firm where the cargo has been other than tea; they confirm the possibility of what is shown by this.

			Cubic feet.
5 tons	Canes	measuring	764·5
2 „	Chinaware, &c.	„	78·1
27 „	Feathers in bales, bristles, &c.	„	2,030·2
6 „	Waste silk	„	329·9
42 „	Straw braid in bales	„	11,484·8
3 „	Tallow and oil in tubs	„	304·1
1,122 „	Tea	„	100,996·1
25 „	Goat skin rugs in bales	„	787·2
11 „	Small packages of sundries	„	585·3
1,243 „	of 20 cwt.	„	117,361·2
Space in holds occupied by 285 tons of shingle ballast		„	8,555
			125,916

Which was stowed as follows:—

	Cubic feet.	
Holds (capacity from builders)	116,320	
Spare cabin	2,500	
Cross bunkers, abaft engines	3,960	
Sail room and space forward	600	
		123,380
Difference		2,536

The measurements for capacity were taken from the inside of the ceiling and sparring to the underside of the beams and not to the deck.

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In coming to speak of more general and in some degree of more practical matters, I will first draw your attention to three diagrams, the nature of which is now familiar to many members of this Institution. The diagrams form part of the extensive information which it is the policy of Messrs. William Denny and Brothers to put into the hands of owners, and through them of all who have to do with the vessels built by the firm.

These three diagrams are for a three-decked steamer. Fig. 2 (Plate IX.) refers to trim, and shows how it is affected by variation in the rate of stowage of the cargo taken on board. It supposes that cargo to be homogeneous in character, and stowed uniformly throughout the steamer in proportion to the measurement capacity. I may say here that the cargo we assume, for the diagrams of this and other vessels, is invariably homogeneous; in the future this rule may be very much modified, but for the present it is adopted as greatly simplifying the treatment of the subject. Fig. 4 (Plate X.) refers to stability, and shows how this can be made sufficient by the introduction of water ballast into the ballast tanks possessed by the vessel. To help in appreciating the arrangements of the vessel the small profile, Fig. 6 (Plate X.) is also added.

The diagram for stability shows that when the steamer is loaded with a homogeneous cargo stowing 61½ cubic feet per ton weight, 370 tons of ballast are necessary for stability when the coal is in, and 420 when the coal is out. For heavier cargoes vacant space becomes necessary in order that the load draught of the vessel may not be exceeded, and this vacant space being left in the between decks less ballast than the above is required. For lighter cargoes somewhat less ballast is also required, and with them the steamer is no longer immersed to her loaded draught.

In fixing the amount of ballast necessary for the stability of the vessel, the assumptions made were that in no condition of her loading should any of the three quantities, metacentric height, righting arm at 30°, and righting arm at 45°, be less than ·8 ft. We preferred to fix the angles at which the righting arms should be considered instead of dealing with the maximum righting arm, to insure more precision in the work; the actual position of maximum righting arm being a good deal dependent on minutiæ of calculation and drawing. The assumptions made are, no doubt, on the high side, and are meant to be so, in every case, until the experience of ourselves and others justifies their reduction. At the fully loaded draught they insure a metacentric height of 1·5 ft., a maximum righting arm of ·93 ft. at 54°, and a range of stability of 93°. The curve of stability for this draught is given in Fig. 7 (Plate X.) The assumptions further involve that with decrease of draught greater range of stability ensues, but a decrease of metacentric

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height; this continues until the metacentric height becomes ·8 ft.; with further decrease of draught this ·8 ft. G. M. is not allowed to decrease.

It will be seen from the diagram that more ballast is required when the coal is out than when the coal is in. In the interesting work which Mr. Nicholson will bring before you, he shows how, for any draught of a steamer, to arrange the bunkers so that this discrepancy may not occur, and he discusses the advantages to be derived from making the ballast, necessary for stability, the same whether the coal is in or out.

Instead of using ballast, it is possible to get sufficient stability by leaving empty space in the 'tween decks. For vessels having a single bottom we calculate the alternate methods, but for steamers with water ballast tanks we do not consider the empty space method likely to be used when a full cargo can be obtained.

In the diagram showing how the trim varies with increasing rates of stowage, I would direct your attention first to the effect of burning out the coal. In the steamer to which the diagram applies this effect is always to put her more by the head. It can within certain limits be made what is wished by a judicious arrangement of the coal bunkers in a fore and aft direction. The trim itself is not so easy of alteration, although for the heavier cargoes it would be well if it were. The diagram under discussion shows that the trim of the steamer varies from 18″ by the stern in the light condition to 10″ by the head in the load condition, the rate of stowage in the latter—if it were possible to do without ballast—being 52½ cubic feet per ton weight. Cargoes stowing less close than 52½ cubic feet to the ton weight still put the steamer considerably by the head; this can, no doubt, be partially remedied by filling up aft the water ballast actually required for stability purposes, but when all the water ballast is so required this remedy becomes impossible. When a cargo is not likely to be homogeneous the trim can be regulated by placing the heavier portions aft, and this is, no doubt, what would generally happen. I have had the effect calculated of moving the machinery one-tenth of the length of the ship further forward, to try whether this movement of the relatively light portion would not improve matters; but owing to the side houses and casings which would also have to be moved, the gain at the load draught would, with coal in, be nothing, and with coal out only about 2½ inches.

To compare with diagrams 2 and 6 I give also diagrams 3 and 5 set off in just a similar manner for a well-decked steamer. It will be seen that for this steamer the trim is always by the stern whatever the rate of stowage of homogeneous cargo; there seems no doubt that this is one of the reasons of the favour obtained for this class of steamers.

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In stowing heavier goods aft for purpose of trim, care should be taken not to cause undue strain on the vessel. For steamers over 350 feet in length we always calculate the longitudinal strength on the assumption that the steamer is fully loaded with a homogeneous cargo and placed upon the crest of a wave having a height $\frac{1}{20}$th the length of the steamer and a length equal to the length of the steamer; in doing this, we neglect allowance for the wave structure, the effect of which was so ably pointed out by Mr. W. E. Smith in his Paper of 1883, and we also neglect the assistance rendered to the section by the wood of the decks, ceiling, &c.; the tensile stress obtained under these restrictions we generally limit to 10 tons per square inch for steel, of which the ultimate strength is about 29. This should represent the very worst that can happen to the steamer and the loading should be so conducted as to reduce this figure rather than increase it.

At the present time, when such extraordinary haste has to be made in the discharge of a steamer, it is necessary that all arrangements should be carried out with a view to this end. To discharge 2,000 tons in less than 24 hours, as has sometimes to be done, the appointments about a hatch must be adequate, and well proportioned. I understand that much time is sometimes lost by one of the holds taking longer time to discharge than the others, and delaying in consequence the final operations upon the steamer; this delay depends very much upon the nature of the cargo as well as upon the arrangement of the holds, and the matter should therefore receive careful attention at the hands of both owner and builder, at the time the plans are being got out. When cargo ports are fitted in the 'tween decks to assist in loading and discharging, these should be placed so that the cargo passed through them shall not interfere with the cargo passed through the hatch and over the upper deck.

In discharging a grain cargo in bulk, I believe delay and annoyance is often caused by the shifting boards in the way of a hatch being not easily removable; such boards should have all the same length, the length indeed of the hatch, and should be so fitted as to be easily got out and subsequently replaced; when the boards exceed the length of the hatch, the ends of them lie buried in the grain standing at a higher level than that in the clear of the hatch, and require time and labour for their excavation. The position of stanchions in the way of the hatch may also cause serious delay, if they are not well spaced, when the cargo to be discharged is of large dimensions.

For rapidity of discharge the size and spacing of the hatches also require careful attention. Where, as in London, the size of the barges is so regular, the spacing of the hatches may be fixed with reference to them. The larger size of barge is, I understand,

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	DEPTHS TAKEN IN HOLDS.								'TW (Same	
	Beams on *alternate* Frames.				Beams on *every* Frame.				Beams on *alterna* Frames.	
	From top of Ceiling to top of Deck Beam.	From top of Ceiling to ⅓rd round down from top of Beam.	From top of Ceiling to bottom of Deck Beam.	From top of Ceiling to ⅓rd round down from bottom of Beam.	From top of Ceiling to top of Deck Beam.	From top of Ceiling to ⅓rd round down from top of Beam.	From top of Ceiling to bottom of Deck Beam.	From top of Ceiling to ⅓rd round down from bottom of Beam.	From top of Wood Deck to underside of same.	From to Wood D underside Beam

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66 ft. in length, and carries from 60 to 70 tons weight; the smaller is about 45 ft. ries 50 tons weight. To Mr. Brittain, of the Millwall Docks, I am indebted for g out a steamer that he found well arranged for working overside into the barges, h the length of the main hatch was 24 ft., the length of the fore hatch 12 ft., and ce between them 28 ft.

nother point of importance in connection with rapidity of working is the relative age of winch and crane and of hydraulic power for loading and discharging. tter has been so successful at the docks in London, and seems to promise so well on ship, that it may almost be called the power of the future. For the comparison of and crane differences of opinion exist among those who have had the practical ing of them. One prefers the crane on account of the greater steadiness with which be worked in a sea-way; with the load close up to the head of the jib, and the whole r control of the craneman, there is nothing to swing; this is not the case with the ck. Another, in the Indian trade, objects to it because if his Coolie craneman gets tened and forsakes his post, the crane is left to swing about quite at its own pleasure. advantage, in point of rapidity, that a double winch has over two cranes is, that n all the cargo is being worked over one side of the ship, the two winches can be ed while the two cranes cannot; this supposes there to be a suitable arrangement of icks or slings. On the other hand, the crane has the advantage of being nearer to the s side, and in consequence of being better able to swing the cargo clear of the side.

For rapidity and convenience in loading operations, the arrangement of the coal ports shoots is a very important item. Trusting, that such a catastrophe as that which happened he *Austral* may never occur again, owners find that for many stations it is desirable to have coal ports as near to the water line as possible for convenience of coaling from barges gside. The position best suited, both for convenience and for safety, and the irability of carrying the coal shoots to the deck should be discussed during the gress of the design.

For the purpose of preventing damage to grain, arising from the heat of the boilers, r. Brittain has favoured me with the following suggestions: That the bulk head separating e boiler room from the reserve bunker or No. 3 hold, should be lined with wood and de grain tight, leaving a space of four inches between the lining and the iron bulkhead; om this space two ventilators should be led, one on each side, up above the bridge or eck-house, which is in nearly every case at about this part of the ship. Speaking from

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experience, in the discharge of some thousand ships annually, he finds that in more than half that number grain from the hold mentioned is more or less damaged.

In the discussion following Mr. Dunn's Paper on Bulkheads, read at this Institution two years ago, a good deal was said about the length of holds required for convenient stowage of cargo. Mr. Raylton Dixon spoke very strongly of the importance of considering the length of the holds with regard to the sizes of goods to be carried, and instanced cases of 40 ft. girders or rails as limiting the sub-division of a small steamer. Everyone was agreed that the holds should be sub-divided as far as ever it is possible without interfering with the stowage of the goods intended to be carried. I suppose we should all like, with Mr. Dunn, to see such a sub-division that two adjacent holds might be filled, and still no fatality follow; waiving this it has seemed to me strange that while surplus buoyancy is so universally considered in relation to freeboard, no attempt has been made to relate freeboard at ends of ship with the lengths of the holds nearest to the ends. I believe a tolerably simple rule could be framed connecting these two together, and requiring, for instance, a freeboard at the fore end of 1 foot for so many feet of length of forehold. Disturbing elements which would prevent the constancy of such a figure, are the position of the foremost bulkhead, the relation of draught to length, and the fineness of the lines. The effect of these three disturbing causes could, I believe, be investigated; but with a view to approaching the subject from the practical side, I should like to see trustworthy records kept in cases of collision, of the length and exact position of the holds injured, the ship's draught of water, the height to which the bulkheads were carried, and the height of the ends of the ship out of water.

In conclusion, I must apologise for the necessarily incomplete nature of the matter I have brought forward; it is but a very small contribution to a very large subject. I can only hope that others will pursue it also, and that the problems of the subject which affect owners and builders jointly may become better understood and more completely solved.

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APPENDIX Ia.

Analysis of Methods Employed by Twenty Different Firms of Shipbuilders in Measuring Capacity of Bunkers.

FIRMS.	Depth Measurement.				Breadth Measurement.					Statement of Capacity.		No. of Cubic. Feet assumed per Ton.	Planimeter. (This column applies to Cargo as well as Bunker capacities.
					Without Ceiling.			With Ceiling.					
	From top of Ceiling to top of Deck Beam.	From top of Ceiling to ⅓rd round down from top of Beam.	From top of Ceiling to bottom of Deck Beam.	From top of Ceiling to ⅓rd round down from bottom of Beam.	To inside edge of frame.	To middle of frame.	To outside edge of frame.	To inside of Ceiling.	To outside of Ceiling.	In Cubic Feet.	In Tons Weight.		
1		—				—			Middle of ceiling.	—	—	44	Used.
2	—				—			—		—	—	42	Used almost exclusively.
3		—		If beam on every frame.	—			—			—	45	Not used.
4		—				—		—			—	43	Used.
5			—		—			—			—	44	Used for approximate calculations.
6													
7				—		—		—			—	45	Used to check.
8				—	—			—		—		45	Used in cases of urgency.
9		⅓ the depth of beam.					One inch in.	—			—	45	Used.
10		—			—			—		—	—	42	Used to check.
11			—		—			—		—	—	44	Used for everything ex. tonnage.
12		—			—			—			—	45	Used.
13		—			—			—			—	(side) 44 (cross) 45	Used to check.
14			—		—			—			—	45 to 40	Used.
15				—	—			—		reserve space.		45	Used.
16				—			—	—			—	43	Used occasionally.
17			—			—		—		—	—	42	Not used.
18			—			—		—			—	44	Used.
19				—	—			—			—	45	Used.
20			—			—		—		reserve space	—	44	Used

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APPENDIX II.

Rate of Stowage of various Materials, giving the Source of the Information.

Scotch Coal.

Ell triping (Hamilton district) 45·6 cubic ft. = 1 ton weight, obtained at the pit by the Allanshaw Coal Company, from 3 trucks weighing about 6 tons each. Screening increases this rate about 10 or 11 per cent.

For similar triping, transmitted by rail, 39·7 cubic ft. = 1 ton weight, as obtained at Wm. Denny and Bros., from 8 trucks weighing from 5 to 6 tons each.

Anthracite coal, from Slamannan district, 39·5 cubic ft.=1 ton weight, obtained from 2 trucks at Wm. Denny and Bros.

Splint coal (large lumps) from Wishaw, 42 cubic ft. = 1 ton weight, obtained from 2 trucks at Wm. Denny and Bros.

The coal weighed at Wm. Denny and Bros. was mostly small, and probably for this reason stowed closer than the average.

Welsh Coal.

Powell's Dufferin
Nixon's Navigation
Ocean Merthyr
Ferndale Merthyr
Hood's Merthyr
} 40½ to 41 cubic ft. = 1 ton weight.

Obtained by taking 10 ton trucks of large coal, levelling and weighing.

This information has been kindly supplied by Mr. Guthrie, of Cardiff.

Newcastle Coal.

Steam Coal { Unscreened, 44 cubic ft. = 1 ton weight. Screening increases this about 8 per cent.
" 42 cubic ft. = 1 ton (Stevens p. 118.)

Stevens says that steam-coals being harder than house do not become so much broken and occupy relatively less space.

Manchester Bales.

The following list gives rates of stowage deduced from an average of bales, kindly supplied by the firms noted, through Mr. H. H. West.

	Messrs. Ismay, Imrie & Co.	Messrs. E. Bates and Sons.		Messrs. F. Leyland and Co.	Messrs. Jas. Moss and Co.	Messrs. Henderson Bros.	
Yarn goods		49	80—160	80	41	59 to 67	Cubic ft. = 1 ton weight.
Piece goods	88	50	40—80	60	54	57 (cases) 43 to 47 (bales)	

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Some of the bales contained about 10 cubic ft., others 50 cubic ft.; the smaller having sometimes, but not always, the higher stowage rate. This rate depends very much upon the market for which the goods are packed. One of the above firms adds a note, indicating that, in stowing, space is lost varying from 12 per cent. in the 'tween decks to 28 per cent. in the after hold; others give a less percentage of loss, and less variation.

Turkey Red Goods.

Yarn	46* to 68	cubic ft. = 1 ton weight.
Cloth boxes ...	80 to 88	

* This figure was obtained from "weighted" goods. The information has been kindly furnished by Mr. Norton, at Wm. Stirling & Sons, Vale of Leven.

Pig Iron.

Stowed as close as possible, wood packing being used in quantity only sufficient to keep pile up. 935·47 cubic ft. = 102 tons weight. Hence 9·17 cubic ft. = 1 ton. This weight and measurement was taken at Wm. Denny and Bros.

Alkali (Newcastle).

47 cubic ft. = 1 ton weight. (Stevens, p. 67.)

Wheat in Bulk.

Stevens gives for the lightest wheat, Egyptian, an average of 57 cubic ft. = 1 ton weight (50 lbs. per bushel); for heavier wheats, such as English, Baltic, Black Sea, American, an average of 48 cubic ft. to 1 ton (60 lbs. per bushel.)

A specially heavy specimen from Adelaide (Messrs. Galbraith, Pembroke & Co.), 40 cubic ft. = 1 ton weight.

The London Dock Companies give—

Surrey Commercial	47·5 cubic ft. = to 1 ton (60 lbs. per bushel).
London and St. Katherine's ...	45 cubic ft. = to 1 ton weight.
Gun Wharf, Wapping	44 cubic ft. = to 1 ton weight.

In bags, wheat takes a little more space.

Flour.

43·2 to 46·7 cubic ft. = to 1 ton weight.

Obtained by Messrs. Wilson, Meyer & Co., Liverpool, from 60 and 30 bags weighed and measured.

Maize.

Is given by Stevens at about 48 cubic ft. = 1 ton weight (60 lbs. per bushel) for a mean of the heavier sorts; by the London and St. Katherine's Dock Company at 46 cubic ft. per ton weight.

Barley.

Stevens gives a variation from 64 cubic ft. = 1 ton weight (45 lbs. = 1 bushel) to 53 cubic ft. = 1 ton (54 lbs. = 1 bushel).

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The London Dock Companies give—

Surrey Commercial 60·3 cubic ft. = 1 ton weight (48 lbs. per bushel); London and St. Katherine's, 58 cubic ft. = 1 ton.

Oats.

Stevens gives a mean of about 72 cubic ft. per ton (40 lbs. per bushel).

The Surrey Commercial Dock Company gives 73·7 cubic ft. (38 lbs. per bushel).

The London and St. Katherine's Company gives 70 cubic ft.

Rice in Bags.

Rice varies according to the amount of paddy or grain in husk, and according to the extent to which the bags are filled.

The London and St. Katherine's Dock Company give 50 cubic ft. = 1 ton weight.

The Gun Wharf give 42 cubic ft. = 1 ton weight.

Messrs. Galbraith, Pembroke & Co. have had experience of a range from 42 to 58 cubic ft. = 1 ton weight.

Tea.

Figures deduced from the Table given p. 666 of Stevens.

Description.	Cubic feet to 1 ton weight.
Congou, chests	95·0
„ half-chests	106·2
Souchong, chests	95·0
„ half-chests	104·6
Orange Pekoe	86·2
„ catties	83·0
Caper, catties	83·0
Young Hyson, half-chests	107·2
„ catties	101·8
Gunpowder, half-chests	99·2
„ catties	93·3
Imperial, half-chests	110·2
Twankey, chests of	111·9
„ half-chests	150·0
Hyson, chests	119·7
„ half-chests	133·8

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Furnished by Messrs. Galbraith, Pembroke & Co.

India tea 85 cubic ft. = 1 ton weight.
China „ 90 „ „

Sugar (Raw).

In baskets 46·1 to 52·8 cubic ft. = 1 ton weight (Java).
In bags 41·1 „ „ (Demerara).

Kindly furnished by Messrs. Galbraith, Pembroke & Co.

Cotton.

The following list gives rates of stowage of cotton deduced from an average of bales, kindly supplied by the firms noted, chiefly through Mr. H. H. West:

	Messrs. Galbraith, Pembroke & Co.		Messrs. Ismay, Imrie & Co.	Messrs. E. Bates and Sons.		Messrs. F. Leyland and Co.	Messrs. Jas. Moss and Co.	Messrs. Henderson Bros.	Stevens (stowage)	
American .		134	109		78·6	106	129		126 to 131	Cubic ft. = 1 ton weight.
Egyptian .		70	71		78·6 to 221·0	65	64		247	
E. Indian .	64 to 111*	60	61	54	73·6	50	61·8	65·3	50 to 87·7	

* 10 Bales weighed and measured.

The differences in stowage are chiefly due to differences of pressing to which the bales are subjected.

One of the above firms adds a note indicating that, in stowing, space is lost varying from 4 per cent. in 'tween decks to 20 per cent. in the after hold; another states that from "Bombay, as a rule, there is "little or no room actually lost by broken stowage, suitable small cargo usually being obtainable to fill "up all vacant space."

Jute.

49·0 to 77·6 cubic ft. = 1 ton weight. (Mr. Carmichael, Greenock.)

The lighter being hand-pressed, the heavier hydraulic-pressed.

Wool.

New Zealand. Messrs. Galbraith, Pembroke & Co.	*Cape of Good Hope.* Mr. George Scott, at Messrs. Donald Currie & Co.
Dumped, greasy 84 cubic ft. = 1 ton weight.	Pressed and bound, greasy 188 cubic ft. = 1 ton.
„ scoured 100 „ „ „	„ scoured 280 „ „
Undumped 235 „ „ „	

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The New Zealand figures were obtained from unbroken tiers of wool, as actually stowed, measuring 690 cubic ft., 561 cubic ft., and 318 cubic ft. respectively. The Cape figures were obtained from a considerable number of actual bales, with a note that stowage space required is some 10 per cent. more than the above.

Potatoes in Bulk.—American.

48 to 51 cubic ft. = 1 ton weight. (Stevens, p. 59.)

Bacon and Ham in Cases.

About 64 cubic ft. = 1 ton weight. (Stevens, p. 73.)

Peas and Beans.

43 to 53 cubic ft. = 1 ton weight. (Stevens, p. 279.)

Beef and Mutton.

As noted in Paper.

The information has been kindly furnished by the Bell-Coleman Refrigerating Company.

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APPENDIX III.

TABLE showing Total Quantity of EXPORTS and IMPORTS of the United Kingdom in the year 1883, with the percentage shipped from or to the Ports noted, taken from the Annual Statement compiled in the Custom House.

	Total Tons.	Percentage of Total.				
		London.	Liverpool.	Newcastle.	Glasgow.	Cardiff.
EXPORTS.						
Coal, cinders and patent fuel	22,800,000	·25	2·80	21·30	2·00	30·60
Manufactured cotton (yards)	4,538,888,500	13·30	69·80	·08	4·30	
Cotton yarn	118,000	14·30	31·40	·41	7·30	
Pig iron, bar, bolt and wire	1,910,000	4·20	17·40	4·20	12·00	
Railway and other iron	2,130,000	17·00	30·60	1·00	9·30	
Alkali	347,000	1·20	56·90	31·40	2·90	
IMPORTS.						Leith.
Wheat	3,210,000	25·30	26·90	1·20	3·00	2·99
" meal and flour	816,000	17·90	26·80	5·20	26·00	5·94
Maize	1,590,000	12·00	30·60	1·34	9·00	·96
Barley	823,000	16·00	1·50	·85	3·00	7·51
Oats	757,000	62·00	·92	1·60	·42	4·46
Rice	387,000	52·50	45·80	·17	—	
Tea	99,200	99·95	·01	—	—	
Iron ore	3,190,000	·12	2·10	9·30	8·80	
Iron and steel goods	194,000	45·70	7·88	1·35	1·68	Greenock.
Sugar (raw)	1,020,000	39·30	27·40	—	·52	6·63
Cotton	774,000	6·60	92·20	—	·02	
Cotton seed	248,000	12·20	8·90	—	—	
Jute	369,000	37·90	3·90	—	·71	
Wool	220,000	80·00	8·50	0·30	·03	
Potatoes	257,000	27·80	1·90	1·90	—	
Bacon and hams	184,000	14·00	61·40	2·20	6·40	
Peas and beans	274,000	17·90	38·50	1·13	12·80	
Copper	199,000	7·91	44·30	6·32	—	
Bar iron	123,000	40·10	7·17	1·10	2·24	

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METHOD OF ARRANGING THE COAL BUNKERS OF A STEAMER SO AS TO REDUCE THE BALLAST TO A MINIMUM.

By JOHN NICHOLSON, ESQ.

[Read at the Twenty-sixth Session of the Institution of Naval Architects, March 26th, 1885; the Right Hon. the EARL OF RAVENSWORTH, President, in the Chair.]

THE object of this Paper is to prove that the position of coal bunkers in steamers should not be arbitrary, and that by judicious arrangement much ballast may be saved, especially in steamers consuming large quantities of coal.

For example, the steamer to which Fig. 2 (Plate XI.) refers, requires actually at the load draught 260 tons ballast. If the centre of gravity of coal were 16·33 feet above keel, 30 tons ballast would be sufficient, and the steamer could carry 230 tons more cargo without increase of draught.

It is well known that in steamers having their coal low, the centre of gravity is raised when the coal is consumed.

In case of a single bottom ship, as much ballast must be taken at starting, as is required to give sufficient stability with coal consumed. An excess of ballast is therefore carried at starting.

If the coal is stowed high, the ballast is in excess when the coal is out. There is a height of centre of gravity of coal, between these two extreme limits, which requires a minimum ballast. This height can be calculated in the following way :—

Let W_o be the displacement of the ship light.
h_o the height of C.G. of ship light.
W_{car} the weight of cargo.
h_{car} the height of C.G. of cargo.
W_{co} the weight of coal.
h_{co} the height of C.G. of coal.
W_s the weight of stores and fresh water.
h_s the height of C.G. of stores and fresh water.
B the ballast.

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h_b the height of C.G. of ballast.

h the height of C.G. of ship for this displacement, meaning that particular height of C.G. which should not be exceeded by the C.G. on account of the safety of the ship. It is here termed the *safe height* of C.G. of ship.

Then we have for the weights and moments the following formulæ, respectively:

$$W_o + W_{car} + W_{co} + W_s + B = W \qquad (1)$$
$$h_o W_o + h_{car} W_{car} + h_{co} W_{co} + h_s W_s + h_b B = hW \qquad (2)$$

Denoting by W^1 and h^1 the displacement and the safe height of C.G. of ship when the coal, stores, and fresh water are consumed, and by B^1 the necessary ballast, we get two similar expressions, viz. :—

$$W_o + W_{car} + B^1 = W \qquad (3)$$
$$h_o W_o + h_{car} W_{car} + h_b B^1 = h^1 W^1 \qquad (4)$$

h^1 is generally different from h because the safe height depends on the draught, besides depending on the conditions laid down as considered necessary for the safety. In the construction of the accompanying diagrams a minimum metacentric height of ·8 ft., and a minimum righting arm of either ·8 ft. or 1 ft. at 30° inclination, and either ·8 ft. or 1 ft. at 45° inclination, have been used as the standards of safety.

There are four unknown quantities in the above expressions, viz., B, B^1, W_{car}, W^1, and they can be calculated as the number of equations is equal to the number of unknown quantities.

This has been done for different values of h_{co} and the resulting values of B and B^1 have been set off as ordinates of two curves, the abscissæ being the values of h_{co}. (Fig. 1, Plate XI.) The two curves are hyperbolæ, as could easily be demonstrated. Their asymptotes are parallel to the axes of co-ordinates.

If there are no ballast tanks in the ship—as in the case referred to in Fig. 1—as much ballast has to be taken in before starting as is ever likely to be required. The actual height of C.G. of coal in the ship is 11·75 ft., and the diagram shows that 260 tons ballast—that is the larger of the two ordinates at the abscissæ 11·75—has to be taken in, which is just the amount required at the end of the voyage, but which is far in excess of the requirements at the beginning. As the *larger* of the two ordinates at the same abscissa shows the ballast which is really wanted, it is evident that the one particular abscissa, viz., 16·33, at which the ordinates of the two curves are of equal length, involves the minimum

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amount of ballast. Analytically, we find this most advantageous height of C.G. of coal by putting in the formulæ (1), (2), (3) and (4)

$$B = B^1$$

and calculating h_{co}, eliminating at the same time the unknown quantities W_{car} and W^1. Thus we find

$$h_{co} = \frac{(h - h^1)W + (h^1 - h_s)W_s + h^1 W_{co}}{W_{co}} \qquad (5)$$

If the righting arms at 30° and 45° inclination are not known, the position of bunkers may be fixed by the height of metacentre; thus, in such cases, we may say: *the centre of gravity of coal bunkers should be one foot below the metacentre* at load draught.

Fig. 2 (Plate XI.) shows the ballast required at various draughts for two different arrangements of bunkers, the one being the actual arrangement, with h_{co} =11·75 ft., the other an arrangement such as to comply at the load draught with formula (5). (The vessel is supposed to be completely filled with a homogeneous cargo.) From the two curves it is obvious that the latter arrangement would be superior to the actual as regards ballast, not only at the load draught but also at any other draught.

Fig. 3 (Plate XI.) shows an arrangement of coal bunkers complying with the formula (5).

For a given height of C.G. of coal, several arrangements of coal bunkers may be planned.

In order to raise the C.G. of coal it would be well to carry the bunkers forward of boilers, up to the main deck. (This applies equally to any other bunkers which terminate at the lower deck.)

Fig. 4 (Plate XI.) refers to a double-bottom ship, and is intended to show how the possibility of taking in ballast during the voyage may be taken advantage of for further increasing the cargo-carrying capacity. The point of intersection of the two curves on this diagram gives the minimum ballast, 740 tons, and the corresponding height of C.G. of coal, 19·45 ft. The two curves show that the centres of gravity of coal ranging between 17·75 and 22·1 involve ballast not exceeding the capacity of the tanks, which is 860 tons. An arrangement of coal bunkers giving a height of C.G. of coal outside these limits, 17·75 and 22·1 would make additional pig iron ballast necessary. The height 17·75 will be seen to be the most advantageous. The curve relating to the "coal in" condition, shows that 676 tons ballast are required when starting. With the consumption of coal the stability of the ship decreases gradually, but the standard of stability can be kept up by taking in water ballast (not more than 860 tons will be required in the worst case), and this is always possible, since the weight of coal is in excess of the ballast added on the way. Certain restrictions are, of course, to be made on account of the necessity of having each ballast tank either quite empty or quite full. The point of intersection of the two curves on

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Fig. 4 gives 740 tons ballast, whilst the ballast really wanted at starting is only 676 tons, which means a gain of 64 tons of cargo carrying capacity.

For the purpose of calculating the best height of C.G. of coal by means of the formula (5), the following data are necessary :—

(1.) Load draught.

(2.) Weight of coal, W_{co}.

(3.) Weight of stores and fresh water, W_s, and the height h_s.

(4.) Part of displacement curve for determining the displacement W when the coal is in and the displacement W^1 when the coal is out ($W^1 = W - W_{co} - W_s$). (This latter is only used for getting out the safe height h^1.)

(5.) Part of curve of transverse metacentre and cross-curves of righting arm for 30° and 45° inclination. They are used for finding the safe heights h and h^1 for the displacements W and W^1 respectively. When the height of C. G. of coal h_{co} is calculated, the height of C. G. of cargo can be determined, provided the cargo is homogeneous—

Let V_{car} denote the capacity of the cargo spaces,
V_{co} the capacity of the coal spaces,
V the capacity of both kinds of spaces together, and
h_v the height of C.G. of V.

Then $V_{car} + V_{co} = V$ and $h_{car} V_{car} + h_{co} V_{co} = h_v V$.

Thence $$h_{car} = \frac{h_v V - h_{co} V_{co}}{V_{car}} \qquad (6)$$

The ballast B and the weight of cargo W_{car} are then to be obtained from the following formulæ, which can easily be deduced from the expressions (1) and (2) :

$$B = \frac{(h_{car} - h) W + (h_o - h_{car}) W_o + (h_{co} - h_{car}) W_{co} + (h_s - h_{car}) W_s}{h_{car} - h_b} \qquad (7)$$

$$W_{car} = W - (W_o + W_{co} + W_s + B) \qquad (8)$$

The curve in Fig. 5 (Plate XI.) shows the ballast which is required to give the vessel the standard stability at different draughts ; the bunkers are supposed to be arranged so as to necessitate a minimum ballast for each draught. The corresponding heights of C. G. of coal are represented by another curve.

The ballast curve is intended to point out how deep the vessel, to which reference has already been made in Fig. 4, could be loaded without exhausting her water ballast,

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by a judicious disposition of the coal. The water ballast is, with the actual arrangement of bunkers, just sufficient to make up the standards of stability at the load draught 26 ft. 1 in.; with the new arrangement the load draught could be as deep as 27 ft. 2 ins. without any additional pig iron ballast being necessary. This means a gain of 390 tons of cargo carrying capacity.

By loading the steamer to the draught 27 ft. 2 ins. the stability is not diminished, it is even increased. The ship is always ballasted so as to give a righting arm not less than 1·0 foot at 30° and 45° inclination. The righting moment in case of the deeper draught is greater.

Fig. 6 (Plate XI.) contains all the data for getting out the safe heights.

Suppose it is required to find the safe height at the 26 ft. draught.

The righting arm at 30° inclination must be = 1·0 ft., it is equal to ·56 ft. with a height of C. G. = 20 ft. The height of centre of gravity of ship must be shifted downwards so as to give the required righting arm.

$$\frac{1{\cdot}0 \text{ ft.} - {\cdot}56 \text{ ft.}}{\sin 30^\circ} = \text{amount of shift of centre of gravity of ship} = \frac{44}{\frac{1}{2}} = {\cdot}88 \text{ ft.}$$

Hence the resulting height of C.G. = 20 ft. — ·88 ft. = 19·12 ft.

The righting arm at 45° (with 20 ft. height of C.G.) = ·45 ft.

The righting arm at 45° (with 19·12 ft. height of C. G.) = ·45 ft. + ·62 ft. = 1·07 ft.

Metacentric height = 20·6 ft. — 19·12 ft. = 1·48 ft.

Hence 19·12 is the safe height.

This diagram shows that upwards of 28 ft. draught the righting arm at 45° inclination is deficient, from 28 ft. down to 23 ft. draught the righting arm at 30° is deficient, and at lesser draughts the initial stability only is deficient.

Specimen Calculation.

Steamer with single bottom.

Load Condition—

A. *Coal, fresh water and stores in.*

Draught	25 ft. 3 in.
Displacement	8,950 tons (W)
Safe height	18·65 feet (h)
Weight of coal	797 tons (W_{co})
Weight of stores and fresh water	75 „ (W_s)
Height of C. G. of stores and fresh water	14 feet (h_s)
Height of C. G. of ballast	4·3 „ (h_b)

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B. *Coal, fresh water and stores out.*

Draught	23 ft. 2½ in.
Displacement	8,078 tons (W^1)
Safe height	18·92 feet (h^1)
Height of C.G. of ballast	4·3 „ (h_b)

Light Condition—

Draught	12 ft. 2 in.
Displacement	3,555 tons (W_o)
Height of C.G.	19·08 feet (h_o)

Substituting these values in formula (5) we obtain for the most advantageous height of C.G. of coal—

$$h_{co} = \frac{-\cdot 27 \times 8{,}950 + 4\cdot 92 \times 75 + 18\cdot 92 \times 797}{797} = 16\cdot 33 \text{ feet}$$

Total space available for cargo and coal	= 279,218 cubic feet (V)
Height of Centre of Gravity of this space	= 18·43 feet (h_v)
Capacity of coal bunkers (44 cubic feet to the ton)	= 797 × 44 = 35,070 cubic feet (V_{co})
Height of C.G. of coal	= 16·33 feet (h_{co})
Cargo capacity = $V - V_{co}$	= 244,148 cubic feet (V_{car})

Substituting in formula (6) we obtain for the height of C.G. of cargo (which is supposed to be homogeneous)—

$$h_{car} = \frac{18\cdot 43 \times 279{,}218 - 16\cdot 33 \times 35{,}070}{244{,}148} = 18\cdot 73 \text{ feet}$$

The weight of cargo can only be found after the ballast is determined. This we do by means of formula (7), viz.:—

$$\text{Ballast B} = \frac{(18\cdot 73 - 18\cdot 65)\ 8{,}950 + (19\cdot 08 - 18\cdot 73)\ 3{,}555 + (16\cdot 33 - 18\cdot 73)\ 797 + (14 - 18\cdot 73)\ 75}{18\cdot 73 - 4\cdot 3} = 29 \text{ tons.}$$

Hence from formula (8)—

$$\text{Cargo weight } W_{car} = 8{,}950 - (3{,}555 + 797 + 75 + 29) = 4{,}494 \text{ tons.}$$

The following is a calculation of ballast independent of the formula. It can be taken as a general guide for this kind of work.

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Without using ballast the load displacement is made up thus:

	Weight.	Height of C. G.	Moment.
Ship light	3,555	19·08	67,830
Passengers, crew, &c. .	37	38·00	1,443
Coal	797	16·33	13,015
Fresh water and stores .	75	14·00	1,050
Cargo	4,486	18·73	84,020
	8,950		167,358

Height of C. G. of ship loaded (no ballast in) . . . = 18·70
while the safe height of C. G. is . . . . . = 18·65
The moment should, therefore, be only 8,950 × 18·65 . . = 166,920
instead of 8,950 × 18·70 . . = 167,358
Difference 438

In order to get that moment—166,920—ballast must be introduced and at the same time an equal weight of cargo must be taken out so as to maintain the load displacement 8,950 tons.

If B denotes the weight of ballast (the height of C. G. of which is 4·3 feet) then—

Moment of ballast = 4·3 B to be added
" cargo = 18·73 B to be subtracted
Difference (18·73 − 4·3) B = 14·43 B,

and this must be equal to the above difference of moment 438, that is—

$$14{\cdot}43\ B = 438$$

$$B = \frac{438}{14{\cdot}43} = 31 \text{ tons,}$$

which is the same ballast as found by the formula.

For the "coal and stores out" condition we find in a similar way:—

	Weight.	Height of C. G.	Moment.
Ship light	3,555	19·08	67,830
Passengers, crew, &c. .	37	38·00	1,443
Cargo	4,486	18·73	84,020
	8,078		153,293

Safe height of C. G. = 18·92; 8,078 × 18·92 . . . = 152,830
Difference 463 = (18·73 − 4·3) B

$$B = \frac{463}{14{\cdot}43} = 32 \text{ tons.}$$

As could be anticipated in consequence of the choice of C. G. of coal, the ballast turns out to be the same in the "coal &c. in" and "coal &c. out" conditions.

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DISCUSSION.

Mr. B. Martell: My Lord, I can scarcely express the gratification that I feel on the production of these two Papers. They are really Papers of great practical worth; and I am enabled to speak on this point from the investigations we have recently made with reference to the very important question of stowage, which is now brought before us in this practical shape for the first time in this Institution. The importance of it is more vividly impressed on my mind on account of the action we have recently taken in Lloyd's Register Society with reference to the assignment of load lines. We felt that to assign load lines to some ships would, in some conditions of loading, cause them to be dangerous ships; and therefore, accompanying the certificate of the load line assigned, we furnished the owners at the same time with the conditions of the ship's stability under certain conditions of loading, and in order to do that it was necessary to obtain information with reference to the various specific gravities of the cargoes that these ships were likely to carry; and there was thus brought clearly before me the great difference of opinion existing in different firms with reference to the specific gravity or weight of different descriptions of cargo. The number of cubic feet going to a ton of certain descriptions of cargo we found to vary very considerably from the inquiries we made; and although Stevens's book, which is referred to here by Mr. Purvis, was no doubt a good book for general guidance at one time, we have found it to be very unreliable at the present time for giving the accurate information which we require. In dealing with this important question referred to by Mr. Purvis, I may say that we accompanied the assignment of this load line with the condition of the ship carrying certain homogeneous cargoes varying in specific gravity, and showed them that they could either, as pointed out by Mr. Purvis, leave out a certain portion of cargo between decks to make the ship safe, or, as an alternative, put in so much water ballast in order to accomplish the same end. But in order to do that it is very evident that accurate and detailed information is necessary; and Mr. Purvis has now come forward after an immense amount of labour, which is evident from reading this Paper and the Appendix to it, in a manner which reflects the very highest credit upon him, and for which, I think, this Institution should express their grateful thanks. The subject, as he says, is not exhausted by any means. In fact, if I were to go so far, without, I trust, in any way wounding Mr. Purvis' feelings, it is really the fringe of the question, and I do hope that this subject will be continued by himself and others in order to give us still further information on such an important subject as this. The value of such information as this may be seen if I instance a case where some few years ago two ships were built in the Clyde for the purpose of carrying a definite amount of cargo from one port on the north-east coast—coals, in fact—to London, and the builders specified that these ships should, on their arrival at the port of discharge, put out a certain quantity of cargo. Well, in reality, when the ships arrived there, they did not put out within 15 per cent. of the amount of cargo they had guaranteed. The case came on for trial in the Court of Session at Edinburgh. I was called upon as a witness in that case and investigated the subject; and it was found that in calculating the dead weight carrying capacity of the ship they had taken 40 cubic feet to the ton of coals, whereas we actually found that something like 46 cubic feet were required. If I am not mistaken—I appeal to Mr. White, who will correct me if I am wrong—I think my recollection goes back to this with regard to what we used to do in the Government service, that is, that we used to calculate something like 48 cubic feet where projections interfering with stowage had to be allowed for.

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Mr. W. H. White: With Welsh coal it is taken at about 43 cubic feet now.

Mr. Martell: Of course the description of coal, whether gas coal or coal from the north-east coast, or whether it is Welsh coal, will vary with reference to its specific gravity, and that only shows the great importance of having an accurate and full knowledge of this subject; when I tell you that the builders of these ships, from an imperfect knowledge of the very subject that this Paper deals with had to pay those owners £1,000 on each of those ships on account of not fulfilling their contract, a result which was brought about by their taking an insufficient number of cubic feet to the ton of coals. Of course with reference to the question which Mr. Purvis alludes to here, of taking the beams and stringer plates and matters of that kind into consideration, that is a thing of very great importance and requires very careful attention before you can tell the carrying capacity of a ship. It is customary for owners to obtain from builders the number of cubic feet in each hold of a ship for their guidance; but, as Mr. Purvis says, it is often very inaccurate from not knowing what enters into the question of capacity, that is, where cargo cannot be placed owing to certain projections and what not. That is a matter of great importance which this Paper deals with, and which will enable those who further investigate the subject to know how to extend the additional information they may get. I see here that Mr. Purvis states that in estimating the strength of the ships, that is, steel ships, they generally limit the tensile strain on the upper works to ten tons. I should like to know whether that ten tons means the strain that is brought on the gunwale of the ship when the ship is in her worst condition, that is, when the coals are consumed, we will suppose, at the midship part of the ship, and she is in the very worst condition in which she can possibly be under any circumstances whatever with reference to the length of the wave and the midship portion of the ship.

Mr. Purvis: That is so, Mr. Martell—the worst condition—on the assumption of a homogeneous cargo.

Mr. Martell: I thought so. It is higher than we are at all accustomed to take as being a fair margin of strength. It occurred to me it would be under the very worst condition it is possible to assume in a ship. There is another point, my Lord, upon which I should like to make an observation, because I am afraid it may possibly lead to misapprehension; and that is the remark with reference to the S.S. *Austral*. It says "trusting that such a catastrophe as that which happened to the *Austral* may never occur again." Now, after the extraordinary judgment of the Wreck Commissioner, we know that very wrong views were entertained by many with reference to the sinking of that ship. It has been described as "capsizing," which is a very inappropriate term altogether to use. I will use the proper term and say *sinking*. I have no hesitation in saying, as I expressed in the Court before the Wreck Commissioner, that the sinking of that ship was owing to the gross negligence of those on board; and it was an accident that might occur to any ship, I do not care what, however much stability she may have; if you have holes through the side of the ship, and those holes or ports are near the surface of the water, if there is no one on board when that ship takes in coals or cargo, and it is all put in on one side and is done in the dark, and those who have to put the cargo or coals on board care nothing about it, and those ports are brought to the surface of the water, it is very evident, as a natural consequence, that the water will flow in and the ship will sink. That was the cause of the sinking of that ship. I should like to explain that, for fear that on

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reading this Paper there might be still some misapprehension with regard to the sinking of the *Austral*, Mr. Purvis refers here to the effect of three disturbing causes, which he believes could be investigated, that is, with reference to the bulkheads of ships. If Mr. Purvis was only in the position that I am, and saw the difficulties we have in dealing with the builders and owners of ships with regard to placing bulkheads in order to fit them for the special service for which they might be required, he would see the very great difficulty there is in practically dealing with a subject of this kind. I think he was quite right to bring the subject before us, and, of course, it will meet with the consideration which such an intelligent Paper as this demands. With reference to Mr. Nicholson's Paper I think that is very important. It is a Paper which reflects the very highest credit upon the writer. A young man, as we see, dealing with a subject of this kind that has not been dealt fully with before, is an unusual circumstance, which in my opinion, calls for special remark and special commendation. I think it is a very healthy sign when we see young men of the age of Mr. Nicholson devoting their attention to practical subjects of this kind—really matters of great practical value and interest. I think that it reflects the very highest credit on those young men, and I think it shows the influence of this Society in stimulating them to exert themselves with regard to important questions of this kind. The arrangement of bunkers referred to, carrying coals at the lower part of the vessel, which are being continually consumed from day to day, must after some days' sailing alter the conditions of safety very materially. From an investigation of this subject, in dealing with load lines, it is forcibly brought to my mind that if the owners of some of these ships knew what a small margin of safety they had when their coals are nearly consumed they would be very much startled indeed, and, therefore, I think that a question of this kind is of so much importance that the dealing with it in so able a manner reflects the very highest credit on the writer.

Mr. W. H. White: My Lord, I have a feeling that the Meeting must be very tired indeed of hearing me speak, but I should be sorry that two such Papers as these are, should be passed by with only so brief a discussion. As regards the Paper of Mr. Purvis, no one can read it without seeing what an enormous amount of labour he must have bestowed on somewhat dry but very important details. I know by personal experience how valuable this information must be in calculations connected with the loading and safety of merchant ships. On the one occasion on which I ever appeared in the Wreck Commissioner's Court (and I think it will be the only occasion on which I shall ever appear in the Wreck Commissioner's Court to give evidence), the whole thing really turned on considerations of this kind; and the question in that case more especially was—how much wheat of a given quality could be put into a certain space? The discussion ended by a special commission being sent to San Francisco to make experiments and observations. I am in possession of that information, but I do not know whether it has been available to Mr. Purvis or not. To illustrate further what I mean as to the importance of Mr. Purvis' information, I would say that in the case to which I refer a scientific investigation showed conclusively that with sugar in bags, the ship with 3 feet 6 inches of freeboard only (and she was rather a deep three-decked ship) was really safer when the water was kept from pouring down the hatchways, than she was with 4 feet 9 inches of side and a wheat cargo. To anyone who looks at the Tables which are given in Mr. Purvis' Paper, and sees what is the difference between sugar and wheat, it will be evident what a narrow difference made all the distinction between safety and danger. For that reason, I think we are under very great obligation to Mr. Purvis for having collected these facts,

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and to the gentlemen who have given him the facts; and I do hope they or somebody else will undertake a new edition of "Stevens on Stowage," or some similar book, bringing the information up to the date of our present knowledge, and putting it in a form that will be useful, not merely for stevedores, but for the naval architect, the shipbuilder, and the shipowner in dealing with the question of the safety of ships. With respect to Mr. Nicholson's Paper, I quite agree with the opinion Mr. Martell has expressed. It is an exceedingly good piece of work. In war ships, where we have, of course, to deal with weights which vary between certain limits, it has, as is well known to the Institution, been always the custom to take account of the changes in stability produced by these variations: as for example, the consumption of coal, fresh water, provisions and ammunition. Now in many war ships, not only by intention but as a matter of fact, we have reached such a condition of stowage of coal that the burning out of the coal leaves the centre of gravity as nearly as possible in the same place. Of course I am quite aware that the stability will be governed by other considerations than the vertical position of the centre of gravity, but after all that is a matter of the highest importance in all these investigations into the safety of ships. Mr. Nicholson has here put down the details of his investigation in a form which will, I think, be generally useful to those who, like himself, are engaged upon calculations connected with ship design, and I cannot help saying that I think the Paper is a very great credit to him.

Mr. H. H. West: My Lord and Gentlemen, when Mr. Purvis asked me if I could assist him in collecting some information on this subject of stowage, I formulated a few questions which I put to several shipowners and merchants in Liverpool. They bear a good deal upon and, in a sense, emphasise what Mr. Purvis has drawn attention to in the sentence—"One of the foremost matters in the question of stowage is the complete knowledge of how many cubic feet are available for cargo in any given steamer." One of the questions I put was this—State the tons measurement of 40 cubic feet per ton which could be stowed in 1,000 cubic feet of actual space in the ship, allowing for ordinary broken stowage. I did not define how that 1,000 cubic feet should be measured, but I took it for granted that the shipowners, from their own plans, describing what capacity they had in the various holds, would compare the actual stowage with the nominal capacity they had in the ship. I found that there were considerable variations amongst the different shipowners, and I cannot explain them on any other assumption than that the shipbuilders who had provided them with plans stating the capacity of each hold, had taken different methods of measuring the available space. The unit I gave was, as I have said, 1,000 cubic feet. In this unit of space one shipowner tells me he will be able to stow as follows:—for the 'tween decks, 22 tons of 40 cubic feet; for the lower main hold, 22 tons; for the lower fore hold, 20 tons; for the lower after hold, 18 tons. Another gives me: 23¾ for the 'tween decks; 24 for the lower main hold—a very small difference, 23⅓ for the lower fore hold, and 23½ for the lower after hold. These figures are for American cotton, and I quote them to show how widely these estimates of stowage differ. These discrepancies may be partly accounted for, no doubt, by differences in the practice of different builders as to what parts of the holds shall be excluded from measurement in reckoning up the ship's capacity for cargo; and, I believe, comparatively few shipowners know what has thus been measured into their holds and what has not. I believe this to be more especially the case with regard to the after holds, where a tunnel encroaches very considerably on the space, and where the form of the vessel is such that there are a number of awkward corners and places that are valueless, or nearly so, for stowage. I had

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expected a very much wider difference between fore and after holds in capacity relative to stowage than was exemplified in the instances given to me. Mr. Purvis has not put these figures in his Paper quite in the form I received them in, but I found that the majority of shipowners from whom I enquired put the lower after hold and the lower fore hold on exactly the same footing. I think that is rather a remarkable fact. In reference to the specific gravities of packages of cargo, the matters that were referred to me by Mr. Purvis were cotton and Manchester bales. On making enquiry I found that there was a very great difference in the specific gravity of packed cotton. There is American cotton, there is East Indian cotton, and there is Egyptian cotton, which may be considered the three principal divisions of that staple as an article of cargo. Manchester goods, I was told at once by nearly everybody, it was utterly impossible to class, because they came in all sorts of weights and sizes, and the regular bales are not to be compared with one another on anything like a common footing. But when I took specific markets, the Alexandrian market, and the East Indian market and the American market, I found there was a sort of unity which ran through each market, and, allowing for that, the figures which Mr. Purvis has given very fairly represent the conditions. As an illustration of the irregularity to which I have referred, one firm told me that Manchester yarn goods would range from 80 cubic feet to 160 cubic feet to the ton, and Manchester piece goods from 40 cubic feet to 80 cubic feet to the ton. With such differing figures, unless those who are forming an estimate of the weight of cargo and its distribution have some idea how these things are severally arranged, I am afraid they may be led very considerably into mistake. I mention that as a caution against using, with anything like certainty and absolute reliance, the general figures which Mr. Purvis has deduced from what has been placed before him.

Mr. W. Denny: My Lord, the Institution will understand that as both the gentlemen who have just read Papers are members of my firm's staff, I am not in a position to make remarks appreciative of their work. The largeness of Mr. Purvis' subject may deter members from dealing with it, and carrying it further. It is thorny with details, which are continually varying. The naval architect's data form the very smallest part, the information which has to be obtained from the stevedore and the shipowner, by far the most difficult part of such work. But the members of this Institution will not be turned back from a subject of this kind by its difficulty and complexity. This very difficulty and complexity should brace their energies to grapple with, and to exhaust the subject if possible. But stowage is not a subject which we shall ever fully exhaust, because it is so constantly varying. Stowages, especially of bale goods, do not remain constant—they are varying in every trade, and differently in different trades. Shipbuilders who are interested in their business, should throw themselves into much closer relationship with the shipowners for whom they build, and with the stevedore, and the practical men who handle their ships. It is only by means of practical information obtained from such men, that we can hope to advance the art of shipbuilding from the regions of theory into those of practical success. I am not going to claim any credit for my firm for the work in these two Papers, beyond that of having their authors on our staff. What I claim, however, is that my firm have laid it down as a fixed policy, that no theory shall be put forward in their office, which is not immediately linked to practice. Such a policy is of value, because theory is continually rendered more fruitful by embracing practice, and practice is kept from degenerating into mere rule of thumb by being wedded to theory. I believe in such matters we are wiser and more sensible in the present day than we were in the past. We are less and less inclined in this

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Institution, either to ignore practice, or to underrate the value of theory, and more disposed to keep them in an unbroken union.

Mr. F. P. Purvis: My Lord, Mr. Martell and Mr. White have referred to the amount of labour there has been in my Paper. As I have tried to acknowledge in the Paper itself, I do not claim credit for this, as the labour has been very much the labour of others, and the information has been largely information put at my service by the firm by which I have the honour to be employed. As to Mr. Martell's remarks about the *Austral*, I am obliged to him for making them. I did not wish to give the impression that there was any fault in the ship. I only meant that the nearer the coaling ports are to the water line, the more likely an error of judgment, and the more likely the careless handling of the ship, are to cause such a catastrophe as happened to that ship. With regard to the bulkhead question, I said what I did rather as a feeler, with a view to getting the opinion of gentlemen in this room, and to see whether information of the sort indicated, could not be procured, and, perhaps, in the future worked up into something of practical value. To Mr. West I have to express my great indebtedness for the trouble he has taken in getting me information from gentlemen in Liverpool. With regard to the broken stowage question that he raises, the information which he gave me seemed so voluminous, that I thought it better to condense it—I think I have given the limits that he obtained.

Mr. West: I think you have.

Mr. Purvis: I say of the stowage of cotton, that "Some of the above firms add that, in stowing, space is lost varying from four per cent. in 'tween decks to twenty per cent. in the after hold." One of the figures which Mr. West quoted just now involved a loss of six per cent. I think Mr. West said in the fore hold, and that only an equal amount of loss occurred in the after hold.

Mr. West: Exactly; that is a thing that surprised me.

Mr. Purvis: I had lost sight of that.

Mr. West: I am speaking now of an individual case. Perhaps we are not speaking of the same thing.

Mr. Purvis: I am much obliged to Mr. West for drawing my attention to it. I have only to thank the Institution for the attention with which my Paper has been received.

Mr. J. Nicholson: My Lord, I have to return thanks for the kind way in which my Paper has been accepted.

The President: Gentlemen, I am sure that I may, on your behalf, return thanks to our two young, and let me say eminent, members, for their valuable Papers.

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ON YACHT MEASUREMENT AND TIME ALLOWANCE FOR RACING.

By Captain J. C. Tuxen.

[Read at the Twenty-sixth Session of the Institution of Naval Architects, 26th March, 1885; the Right Hon. the Earl of Ravensworth, President, in the Chair.]

Object of Yacht Measurement and Time Allowance.

It is well known that a larger yacht possesses greater sailing power, or power of obtaining speed, than a smaller one of the same shape; and also that yachts of equal size, but of different form, do not possess equal power of obtaining speed.

Thus it follows, either that different yachts cannot compete together at races in a fair manner, unless they have by nature equal sailing power, or that the yachts possessing greater sailing power than others, have to give a time allowance to these.

The object of yacht tonnage is, therefore, to measure the sailing power which a yacht possesses by nature on account of her dimensions and proportions, and the object of time allowance tables is to subtract from the larger yachts those advantages which nature gives them in power of obtaining speed relatively to the smaller ones.

If the system of measurement is not based on the above principle, it will encourage the building of special types, namely, those that for a given tonnage by that system possess the largest power of obtaining speed. This will be obtained by increasing those elements conducive to speed, which the system taxes too little, at the cost of those which are taxed too much, so that if the type thus produced has to sail under a fair system of measurement, it will not prove superior in speed. It is generally acknowledged that a well designed yacht has to fulfil other very important conditions besides speed, such as seaworthiness, safety against capsizing, room for accommodation, strength of hull, &c.; so that, if the special types encouraged by any system do not satisfy these requirements, the system is bad.

The Elements governing the Speed of a Yacht.

The speed of a yacht depends on (1) the propulsive effect of the sails, and (2) the resistance of the water to the motion of the hull.

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(1.) The propulsive effect of the sails varies directly as their aggregate area.

Hence to obtain equal speed for different yachts, the area of sails of these must be in the same relative proportion as their respective resistances at this speed. If the area of sails is increased within certain limits, it may be approximately assumed that the speed will increase, until the resistance is increased in the same proportion as the area of sails.

Hence we get approximately:

$$\text{Propelling force of a yacht} = c_1 S$$

where c_1 is a co-efficient and S the area of sails.

If it be further assumed that the speed of a yacht varies as the square root of the resistance, we get the speed to vary approximately as $\sqrt{S}$.

(2.) The resistance of the water counteracts the motion of the yacht in such a way, that she will move steadily on at the speed, where the resistance just balances the propulsive effect of the sails.

The resistance depends on the size or displacement and form of the yacht.

The influence of displacement may be examined by comparing yachts identical in form, but of different dimensions. Assuming the resistance to vary as the square of the speed, and using the theory of corresponding speeds given by the late Mr. William Froude, it will be found that the resistance at a given speed of the different yachts in question varies as (displacement)$^{\frac{2}{3}}$. Hence is got:

$$\text{Resistance at a given speed} = c_2 D^{\frac{2}{3}}$$

where c_2 is a co-efficient and D is displacement.

By a similar reasoning we get, when the propelling power remains constant, that the speed of the yachts varies inversely as $D^{\frac{1}{3}}$.

The assumptions made to obtain all the above results, are, however, only approximations, but may be considered sufficiently exact to explain the principle of yacht measurement.

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The influence of form of the yacht on resistance cannot at present be determined exactly by any law, but the influence of the different dimensions and proportions may be discussed, and before being able to determine how they ought to be taxed by the measurement, their influence on the sailing power of the yacht must be examined.

Besides determining the resistance, the size and form of a yacht also determine the propulsive power, as they are in a certain relation to the area of sails, which the yacht is able to carry. This relation is generally expressed by—

$$E = \frac{D \times m}{S \times h}.$$

Where D = displacement, m = metacentric height, S = area of sail, h = height of centre of effort. E is generally understood to express the power of the yacht to keep upright for a wind pressure of 1 lb. per square foot of sail-area, so that the smaller the value of E, the more the yacht will heel over.

The value of E is not a constant quantity for different yachts; generally it is allowed to be lower for smaller yachts relatively to larger ones, and it is also allowed to be lower for yachts with long ranges of stability relatively to yachts with shorter ranges.

The equation may be put thus:

$$S \times h = \frac{D \times m}{E}.$$

When size and form of a yacht are given, the values of D, m and E are given, and consequently the area of sails is determined. When on the other hand only the area of sails is given, there is still a large margin of variation in the size and form of the yacht.

The Influence of the Dimensions and Form of a Yacht on her Sailing Power.

The size or displacement and form of a yacht are determined by the following factors:

$$L \times B \times H \times C.$$

Where L = length on water line, B = breadth on water line, H = draught and C = co-efficient of fineness.

The influence of these factors on the sailing power and other qualities of the yacht is now to be examined, to determine how they ought to be taxed by the system of measurement, starting from a well-proportioned vessel.

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Length L.—By increasing the length, the other factors remaining constant, the power of the yacht to carry sail increases in the same proportion, while the resistance is only augmented to a smaller extent, in some cases at high speed perhaps not at all. Hence the speed increases sensibly with the length, and this has consequently to be taxed by the measurement. Probably the length is the most important dimension for speed.

Breadth B.—By increasing the breadth, the other factors remaining constant, the power to carry sail will increase very rapidly. The resistance also increases, but the proportion of breadth to length which is most advantageous for speed, when the breadth is not taxed, will be too large to satisfy the requirements of safety, seaworthiness, &c. Hence breadth ought to be taxed so much, that there is no advantage by increasing it unduly, but it ought not to be taxed so severely that an undue decrease is encouraged.

Draught H.—By increasing the draught, the other factors remaining constant, that part of the stiffness of the yacht, which is due to her form, is not altered. The weight of the ballast is increased, and its position is lowered, whereby the stiffness due to ballast becomes greater, and the stability at large angles of heel is improved. Thus the stiffness is increased and the value of E (previously defined) is allowed to become smaller, and consequently the power of the yacht to carry sail is increased. The resistance of the water also increases with the draught, and does so more rapidly in a yacht of comparatively short length and large breadth, so that it is chiefly in a long and narrow yacht that great increase of draught is an advantage.

Increase of draught will also increase the lateral resistance of the yacht, which is of great advantage for lessening drift, when sailing close to the wind, and as a rule tacking plays a very important part, and is highly valued at races.

Thus it seems desirable to include draught in the measurement, but it must on no account be taxed too severely, as that will lead to an undue decrease of this dimension which is so important for the safety and seaworthiness of the yacht.

Co-efficient of Fineness C.—This co-efficient only varies a little in yachts that have their dimensions in the same relative proportion. If the co-efficient is increased, or the form made more full than desirable, the resistance of the water to the motion of the yacht will increase more than her power to carry sail; hence there seems to be no necessity for taxing this co-efficient to prevent undue fulness. If, moreover, the co-efficient is measured, this taxing will encourage a tendency towards decreasing the fineness below the value,

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which is the best for the yacht, when her principal dimensions are given, and this will be a disadvantage to several of the requirements the yacht has to fulfil, such as seaworthiness, &c.

Secondly, it has to be considered whether that part of a yacht which lies above the water-line has to be measured. The total volume of the hull is determined by—

$$L^1 \times B^1 \times H^1 \times C^1,$$

where L^1 = total length, B^1 = total breadth, H^1 = total depth, and C^1 is a co-efficient.

The excess of the total length above the length at water-line has probably as much influence on seaworthiness and appearance as on speed. Thus it does not seem necessary to tax this excess of length, except it should prove desirable to prevent excessive rake of stem and counter.

Excess of the total breadth above the breadth of water-line will increase the stiffness of the yacht as she heels, which will be beneficial to her speed, and therefore the largest breadth of the yacht ought to be measured.

The excess of the total depth above the draught determines the height of the deck above the water. This height has in several respects the opposite influence on speed at races as on seaworthiness, safety, room, &c. If the deck-height is increased, the weight of hull is increased, the centre of gravity and the centre of effort are both lifted, all of which tend to decrease speed. If the deck-height is decreased, it may be advantageous to the speed, but it will be a disadvantage to the seaworthiness, safety, and other requirements. Hence it follows, that the system of measurement ought not to tax the deck-height, thereby encouraging low freeboard.

The same remarks as were made to co-efficient C apply to co-efficient C^1.

With reference to ballast, it will be very difficult and complicated to introduce this into the measurement, and it does not seem necessary, as any improvement in the ballast of a yacht whose dimensions and form is settled, such as introduction of lead ballast and lead keel, is a real advantage to the yacht also apart from racing, and ought not to be discouraged by having to be taxed.

Systems of Measurement.

It has been shown that both propulsion and resistance have to be considered by the system of measurement; thus any system that only measures one of them is incomplete.

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Under this head belongs the system of measuring only the area of sail; this system will encourage the form of hull where minimum resistance is obtained for a yacht carrying a given area of sail. This shape of hull will most likely unite comparatively great length and breadth with a small displacement, and therefore want safety, &c. It is often argued that by measuring the area of sail, the method of ballasting is taxed, but this is not so, as improvement in ballasting, such as introduction of lead keel, increases the power of carrying a given area of sail with minimum resistance.

If the theories of propulsion and resistance, as previously discussed, were sufficiently exact, the most direct method would be to measure these, and express the sailing power of a yacht by—

$$c^1 S - c_2 D^{\frac{2}{3}} \text{ or } \frac{c_1 S}{c_2 D^{\frac{2}{3}}}.$$

As formerly mentioned, the propelling power would then vary approximately as S and the resistance as $D^{\frac{2}{3}}$, hence the two terms have equal influence on speed, and as propulsion is taxed and resistance compensated, we get that all yachts for which this expression has the same value, will possess equal power of obtaining speed, and the system will not encourage any special type.

Experience proves, however, that the two terms are not sufficiently exact for practical use, and that especially the resistance for yachts of different form does not vary as $D^{\frac{2}{3}}$, and therefore the system does not appear to be applicable in practice.

Instead of using the expressions of propelling power and resistance, the gain and retardation of the speed itself might be used, and the relative speed of yachts would then be expressed by—

$$c_3 \sqrt{S} - c_4 D^{\frac{1}{2}} \text{ or } \frac{c_3 \sqrt{S}}{c_4 D^{\frac{1}{2}}}.$$

In America a system has been proposed which has much resemblance to this expression, viz.:

$$\text{Number for time allowance of a yacht} = \frac{\sqrt{S} + L}{2}.$$

It is seen that the speed-gaining term in this formula is correctly $\sqrt{S}$. The speed-retarding term D is here replaced by L, but length is not the only dimension on which resistance depends, and another error appears to be that the two terms have got the same sign, while the term expressing the retardation of speed should be negative, to be com-

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pensated instead of taxed. Most likely the idea with this system has been that area of sail and length are the only elements conducive to speed, and are hence those to be taxed. Even if this were so, it is seen that as $\sqrt{S}$ and L are taxed equally, the system favours that special type in which the relation between these is most advantageous to speed. The other proportions of the yacht will be settled so as to cause least resistance. Thus the broad shallow centre board type of yacht seems to be favoured.

The same reasoning will hold for systems where $\sqrt{S}$ and L are multiplied together.

Most systems in actual use consider both the propelling power and resistance by measuring the hull of the yacht, which determines the area of sail and the resistance of the water. The systems following this principle may be divided into four classes, as follows:

Class A.—Systems that express the tonnage by using all the four factors L, B, H and C or L^1, B^1, H^1 and C^1.

To this belongs:

(1.) *The Displacement Rule*, by which

$$\text{Yacht tonnage} = \frac{L \times B \times H \times C}{35}.$$

This system taxes all four factors equally, while their influence on the sailing power of the yacht is very different. Especially L is taxed too little and C too severely. Experience has proved that the system encourages racing machines that do not satisfy the requirements of safety, seaworthiness, &c.

(2.) The system of measuring the total *external bulk* by which

$$\text{Yacht tonnage} = \frac{L^1 \times B^1 \times H^1 \times C^1}{K}.$$

K is any convenient number.

Similarly to the displacement rule this system taxes all four factors equally, while their influence on the sailing power is very different. In addition, it taxes the height of free-board, which has been shown to be a drawback.

Class B.—Systems that express the tonnage by using the three factors L, B and H or L^1, B^1 and H^1. To this class belong—

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(1.) A system proposed by Mr. H. Hansen, of Copenhagen, by which

$$\text{Yacht tonnage} = \frac{L \times B \times H}{100}.$$

This system taxes the three dimensions equally, and therefore breadth, and especially draught, are taxed too much relatively to length. In relation to Class A it is an advantage to this system that co-efficient C is not taxed.

(2.) The *Corinthian and New Thames Yacht Club Rule* by which

$$\text{Yacht tonnage} = \frac{L^1 \times B^1 \times H^1}{200}.$$

Besides suffering from a similar want as the system just mentioned, this system has the further drawback of taxing the height of freeboard.

(3.) The *Girth Rule*, by which—

$$\text{Number for time allowance of a yacht} = L^1 \times G \text{ (or } L^1 + G).$$

Where G is girth. This system belongs to Class B, as $G = B^1 + N \sqrt{(B^1)^2 + 4\,(H^1)^2}$; where N is a co-efficient that varies but little for different yachts.

This system taxes length and girth equally, and will therefore encourage the proportion of these which gives the greatest speed. Secondly, the system taxes freeboard; and thirdly, the relative proportion in which breadth and draught are taxed is not the same as their relative influence on the sailing power.

Class C.—Systems that express the tonnage by using length and breadth of the yacht.

The three most important systems in this class are:

(1.) *Royal Thames Yacht Club Rule*, by which

$$\text{Yacht tonnage} = \frac{(L - B^1)\,\frac{(B^1)^2}{2}}{94}.$$

(2.) *Rule of "Le Cercle de Voile de Paris,"* by which, using English measures,

$$\text{Yacht tonnage} = \frac{(L \times B^1)^{\frac{3}{2}}}{525}.$$

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(3.) *Yacht Racing Association Rule,* by which

$$\text{Yacht tonnage} = \frac{(L + B^1)^2 \times B^1}{1730}$$

The first two of the systems have proved themselves by actual experience to favour comparatively narrow yachts. The third system will only do this to a less extent.

The following Table shows how the breadth is taxed relatively to length by the three systems. As example, is chosen a series of yachts with varying proportions of breadth to length, while the product of these dimensions is constant.

Proportion of Yacht.	Tonnage by the Thames Rule.	Tonnage by the French Rule.	Tonnage by the Association Rule.
Length = 3 × Breadth . . .	102·5	95	89
Do. = 4 × do. . . .	100	95	90·5
Do. = 5 × do. . . .	95·5	95	93
Do. = 6 × do. . . .	89·7	95	95·5
Do. = 7 × do. . . .	86·2	95	99·5
Do. = 8 × do. . . .	82	95	103

It is seen that within the limits of the Table, the first rule taxes breadth more than length; the second taxes both equally; and the third rule taxes length more than breadth, and is thus nearest the truth.

As the systems belonging to Class C do not tax draught, they encourage an increase of this dimension as long as it is advantageous to speed.

Class D.—The system by which only the length of a yacht is measured. This system will encourage an undue increase of the breadth and draught, as these dimensions are given free, while their influence on the sailing power ought not to be neglected.

A theoretically perfect system cannot be formed for practical use, so that every system

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will necessarily favour its special type to a larger or smaller extent. Hence the system to be preferred ought to possess the properties, firstly, that the type it favours unites the condition of speed with the other important requirements of a well-designed yacht; and, secondly, that the amount by which the type is favoured is as small as possible, so that other types are not excluded from competing under the system.

Proposal for a New System of Measurement.

According to the preceding investigations, the principles on which the system of measurement should be based appear to be the following:

(1.) The dimensions to be taxed are L = length at water-line (with probably some addition for yachts with excessive rake of stem and counter), B^1 = largest breadth, H = draught of water.

(2.) Of the total sailing power which a yacht possesses, a certain percentage is due to her length, a certain percentage to her breadth, and the remainder is due to her draught.

(3.) The three dimensions are to be taxed in the same relative proportion as their influence on the sailing power of the yacht.

(4.) By increasing any one of the dimensions the tonnage of the yacht is to increase proportionally to the increase of that part of her sailing power, which is due to this dimension.

To satisfy these requirements the following system is worked out, viz.:

$$\text{Yacht tonnage} = \frac{C_1L^3 + C_2(B^1)^3 + C_3H^3 + C_4L \times B^1 \times H}{C_5}.$$

The co-efficients $C_1 - C_5$ have to be determined so that the above conditions are satisfied, and should this not be possible more terms must be added, but no doubt it will be found that the number given is sufficient.

The co-efficients have been determined to suit several yachts of great variety of form which have sailed so much together that their relative sailing power is known, and the following result has been obtained:—

$$\text{Yacht tonnage} = \frac{L^3 + 10(B^1)^3 + 30\,H^3 + 20\,L \times B^1 \times H}{11{,}000.}.$$

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The following Table shows the relative influence of the three dimensions on the sailing power of some different forms of yachts according to this system.

Proportions of the Yacht.	Percentage of sailing power due to the length.	Percentage of sailing power due to the breadth.	Percentage of sailing power due to the draught.
	Per cent.	Per cent.	Per cent.
L : B : H :: 1 : $\frac{1}{3}$: $\frac{1}{8}$	56·5	28·5	15
L . B : H :: 1 : $\frac{1}{4}$: $\frac{1}{6}$	60	20·5	19·5
L : B : H :: 1 : $\frac{1}{6}$: $\frac{1}{5}$	63	13·5	23·5

The following Table shows how much the tonnage of the yacht increases, when one of the dimensions is increased 10 per cent., the others remaining constant.

Proportions of the Yacht.	By increasing the length 10 % the tonnage ncreases.	By increasing the breadth 10 % the tonnage increases.	By ncreasing the draught 10 % the tonnage increases.
	Per cent.	Per cent.	Per cent.
L : B : H :: 1 : $\frac{1}{3}$: $\frac{1}{8}$	18	9	4·5
L : B : H :: 1 : $\frac{1}{4}$: $\frac{1}{6}$	19·5	6·5	6
L : B : H :: 1 : $\frac{1}{6}$: $\frac{1}{5}$	20·5	4·2	7·5

If this system should be adopted in any place and should be found not to tax the three dimensions according to the experience at that place the co-efficients are easily altered. If any dimension is found to be taxed too severely the corresponding co-efficient has to be diminished, and *vice versâ*. If extreme proportions of yachts are found to be taxed too much the co-efficient of the fourth term has to be increased, and *vice versâ*.

Tables of Time Allowance.

The principles in computing time allowance tables must, like the principles of measurement, be based on experience, as they cannot be determined by theory alone; and

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it is equally important to have correct tables of time allowance as a satisfactory system of measurement.

The principles adopted by the Yacht Racing Association and many other clubs are as follows:

(1.) The allowance of time given by a larger to a smaller vessel should be directly proportional to the distance sailed, and inversely proportional to the size of the vessels.

(2.) It is supposed that a vessel of 30 tons can allow one of 29 tons one second per mile.

From these suppositions the allowance of time for each ton is obtained by the following relation:

$$t = \frac{k \times 30}{T},$$

where t = allowance of time per ton in seconds,
k = length of course in knots,
T = tonnage of the vessel.

The experience obtained in several places has been, that the above principles answer well when the wind is fair, but cause a smaller yacht to be taxed too severely relatively to a larger one, when there is a strong breeze, and too little when there is only a slight wind. According to the experience in several places, the principles should be of the following form:—

(1.) The allowance of time given by a larger to a smaller yacht should be directly proportional to the distance sailed and to the velocity of the wind, and inversely proportional to the size of the yachts.

(2.) It is supposed that a yacht double the size of another can allow the latter 2·6 seconds per mile sailed through, and per knot velocity of the wind.

According to these principles, the following Table has been worked out, and found to answer well. The average velocity of the wind during the race is measured by an anemometer.

The time a larger yacht has to allow a smaller one is found in the following manner: Take the times placed opposite the tonnages of the two yachts; the difference between these times is multiplied by the length of the course in miles and the velocity of the wind in knots, and the product is then the desired time allowance.

The time allowances according to this Table correspond very nearly to the Y. R. A. Rule, when the velocity of wind is 8 knots.

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As the time allowance is based on the distance sailed through by the yacht, it is evident, that for the part of the course through which the yachts have to tack, the distance ought to be reckoned as the way actually sailed through, and this is probably about twice the direct distance between the termini of this part of the course. No estimate seems however as yet to have been taken of this important point.

TABLE OF TIME ALLOWANCE PER MILE (KNOT) LENGTH OF COURSE, AT A VELOCITY OF WIND OF ONE KNOT.

Tonnage.	Time.	Tonnage.	Time.	Tonnage.	Time.	Tonnage.	Time.
	Seconds.		Seconds.		Seconds.		Seconds.
1	0	26	12·23	51	14·76	76	16·25
2	2·60	27	12·37	52	14·83	77	16·30
3	4·13	28	12·50	53	14·90	78	16·35
4	5·20	29	12·63	54	14·97	79	16·39
5	6·04	30	12·76	55	15·04	80	16·44
6	6·73	31	12·88	56	15·10	81	16·49
7	7·30	32	13·00	57	15·17	82	16·53
8	7·80	33	13·12	58	15·23	83	16·58
9	8·25	34	13·23	59	15·30	84	16·62
10	8·64	35	13·34	60	15·36	85	16·67
11	9·00	36	13·45	61	15·42	86	16·71
12	9·33	37	13·55	62	15·48	87	16·76
13	9·63	38	13·65	63	15·54	88	16·80
14	9·90	39	13·75	64	15·60	89	16·84
15	10·16	40	13·84	65	15·66	90	16·88
16	10·40	41	13·93	66	15·72	91	16·93
17	10·63	42	14·02	67	15·78	92	16·97
18	10·85	43	14·11	68	15·83	93	17·01
19	11·05	44	14·20	69	15·89	94	17·05
20	11·24	45	14·28	70	15·94	95	17·09
21	11·42	46	14·37	71	15·99	96	17·13
22	11·60	47	14·45	72	16·05	97	17·16
23	11·77	48	14·53	73	16·10	98	17·20
24	11·93	49	14·60	74	16·15	99	17·24
25	12·08	50	14·68	75	16·20	100	17·28

In conclusion, I should like to mention, that much valuable information for the working out of this Paper has been supplied by Mr. Alfred Benzon, of Copenhagen.

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DISCUSSION.

Mr. W. H. White: My Lord, I do not rise for the purpose of discussing the Paper so much as to say that in Captain Tuxen's absence, I would like to explain the circumstances which led to its being written. Captain Tuxen is an old pupil of mine at the Naval College, where he took high honours, and he is now a Constructor in the Danish navy. He was asked by the Copenhagen Yacht Club to prepare a rule for yacht tonnage, and this Paper is the result of that application. He sent it to me privately, and, thinking it was a subject interesting to many members of our Institution, I asked him to be so good as to send it here. That is the history of the Paper, and I may say that Captain Tuxen would be very much obliged by any criticisms that might be offered by those who understand the subject.

Mr. H. Liggins: I believe the only one in the room at this moment who has had much experience in yacht sailing is myself, for I have been deserted by a very honoured member of the Yacht Racing Association who was in this room an hour ago, and by a great expert in such matters, the Nautical Editor of the *Field*, Mr. Dixon Kemp. Of course it will be impossible for me to criticise the details of this important and most valuable contribution to one of the most difficult subjects that can engage the Naval Architect, but I look with hope and great expectation that some of our young men, who have so thoroughly distinguished themselves in this room to-day by bringing forward very difficult and elaborate subjects with so much skill and success, will give a little of their spare time to the elucidation of the requirements of the yachting community, and therefore to them, from its acknowledged difficulty, one of the most interesting which could occupy their attention for the general good of the community. Yachts are costly constructions. They are often designed by Naval Architects and owners with a view to trying some new principle in the form of vessels before adopting it on vessels of larger size; they therefore deserve to be acknowledged in every possible manner, apart from their national interest and the occupation which they give to thousands of the labouring classes. The Yacht Racing Association has been engaged for some time past in the laudable effort of endeavouring to do what is almost an impossibility, and that is to bring together something like a time allowance and a scale of measurement suitable for the very varied forms and shapes of yachts. I am sorry to say that the labour has been very much increased in consequence of the introduction during the last eight or ten years of a class of racing vessels that are utterly valueless for any purpose whatever except for winning cups. To my old-fashioned mind they are not vessels at all, they are merely racing machines, and if I had a million of money I should feel it a degradation to show the bad taste of owning such a vessel. It means that the rich, purse-proud man can put 1,500 or 2,000 sovereigns of lead on a yacht's bottom, which is not making that vessel a fine vessel at sea, but only a cup hunter. Therefore it is, in my opinion, quite impossible that the old-fashioned cruising yacht, which was such a splendid vessel for all purposes for which it was constructed, and the more modern racing yachts, the fast cruisers of ten years ago, can ever be coupled with these racing machines. Therefore the attempt which this gentleman makes is, to my mind, one of those things which might very well be avoided, because it cannot lead practically to any very valuable result to naval architecture. I dare say there are some good points in this Paper, and I agree with some of the remarks made, and more particularly that our efforts in constructing a yacht should be to

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make her safe. It is generally acknowledged that a well-designed yacht has to fulfil other very important conditions besides speed, such as seaworthiness, safety against capsizing, with moderately high bulwarks for safety of ladies, and general utility, &c. If that was kept in mind there would not be much necessity for altering the rule which was magnificently elaborated by the late Lord De Roos thirty years ago, and which for so many years was the pride of the whole yachting world.

Mr. J. Inglis, jun.: My Lord, there is one statement which I think must be an oversight on the part of a gentleman of Captain Tuxen's abilities. He says "the propulsive effect of sails varies exactly as their aggregate area." I am not sure that such is the case even if the vessel is going before the wind, and I am perfectly certain it is not so when she is on a wind. In that case, at least, the propulsive effect of the sails does not vary exactly as their aggregate area, but increases much faster than the area of the sail. A large sail is much more effective than two smaller ones of the same total area. That is perfectly well known. There is one proposal in this Paper which is remarkable, because it differs from all previous schemes of time allowance, inasmuch as it attempts to take account of the speed of the vessels when sailing over a course. The writer bases his allowance on the tonnage arrived at by an elaborate rule, and also on the real time occupied in sailing the course. That has not been hitherto attempted, and it is a very important point no doubt. I have seen—and I am sure any person engaged in yacht racing has often seen—races which have degenerated into drifting matches, when some of the vessels have arrived within a few yards of the winning flag boat with perhaps half-an-hour's time to run, and have lain there helpless for want of a little wind to bring them up through the line. If the length of time occupied in sailing the course had been taken into account, it is very possible the results might have been altered and the vessels that had sailed to win might have won upon these occasions.

Mr. R. E. Froude: I am sorry, my Lord, that I had not an opportunity of reading this Paper earlier. I have only had time to skim it through, and I have not the faculty of mastering in a moment a Paper which contains so much intricate work. I therefore do not feel myself qualified to compliment the writer as I believe he deserves to be complimented on the amount of careful and scientific research he appears to have bestowed on it, nor am I qualified to express a decided opinion as to whether all the argument in it is perfectly sound. As far as I can judge from a hasty perusal of the Paper, the argument itself is perfectly correct, but in my opinion it is based on a wrong premise. The Paper says: "Thus it follows either that different yachts cannot compete together at races in a fair manner unless they have by nature equal sailing power, or that the yachts possessing greater sailing power than others have to give a time allowance to these." Now it seems that it is not the object that yachts generally should "compete together at races in a fair manner." In my view the object is that the best designed yachts should have the advantage, and, as far as I can see, in so far as the principles on which the writer has proceeded have been successfully carried out, they are calculated to put all the different designs of yachts on a par, and I do not think that is a desirable object. In considering the rules under which yachts are to compete in racing, we have, as it seems to me, two principal objects to consider. First we have to provide good sport. I put that first because the motive which induces people to spend money and time in racing yachts is that of enjoying sport, and however well-considered the rules may be in other respects, and however likely races

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under such rules may be to give valuable information; still if they do not produce good sport we shall not have many races. Therefore, to produce good sport is the first and most essential thing. But the second point is that the money and time which are spent by yacht owners in racing should, as far as possible, be utilised to give us information as to the best form of vessel for purposes other than racing, and in saying this I conceive that I am quite at one with Mr. Liggins. But I differ from him as to the suitability of the present form of racing yachts to purposes other than racing. I confess that I am not qualified to speak authoritatively on this point from personal experience, but judging from the opinions of many yachting men with whom I have mixed, I believe that the modern racing yachts are exceedingly good sea boats, have very large accommodation in proportion to their nominal tonnage, and have not only high speed but good accommodation in proportion to their sail-carrying power; and the best test, or perhaps I should rather say the only satisfactory test, which racing affords of the serviceable capabilities of a yacht is that of her speed relatively to the money that she costs to work—namely, the number of hands she requires—and it seems to me that the best rules you can devise are those which will, as far as possible effect, that object. The sail area rule which has been in the Appendix of the Yacht Racing Association for some two or three years is certainly a step in the right direction. There are several objections to it, and one which appears to me to be a very valid objection is that it is likely to encourage an excessive area of mainsail in proportion to other sails. There are also other objections, and perhaps all these can be got over by some modification of the rule; but my own impression is that the simple measurement of stability would be the best test. There is this to be remembered, that in so far as you can contrive that the yacht which races most successfully shall be also the most practically serviceable boat, you will be so far also contributing towards sport, because the condition of successful competitive sport is that you should have plenty of competitors, and the more nearly the most successful racing yacht corresponds to the most serviceable yacht for other purposes the more competitors you are likely to have. The method of this Paper, which as I understand it, aims at putting all yachts, of whatever design, on a par, no doubt tends to provide plenty of competition, but my quarrel with it is, that it pursues this object to the neglect of the other very important object, namely, that of making yacht racing a school for yacht designing in general. I have a few words to say with reference to the question of time allowance. It is rather a curious coincidence that Captain Tuxen should have just now presented this paper in which he suggests that the speed of the wind in a race should be measured by a wind gauge, because it so happens that I wrote to Mr. Dixon Kemp during this winter making the same proposition. The author of the Paper has recognised that the speed of the wind is a very important factor in determining the relative speeds of vessels of different size. In a very strong wind the speed of the wind may safely be said to bear about the same relation to the speed of the vessel in large yachts as in small ones. But in proportion as the wind is light so the relation of the speed of the wind to the speed of the vessel becomes smaller in large yachts than in small ones, and the proportionate excess of speed of the larger yachts is diminished. To diminish the time allowance on this score will be correct if the time allowance is made proportional to the duration of the race, but not, as it seems to me, if it is made proportional to the length of course. *Prima facie* a time scale based on the duration of the race is much more rational than one based on length of course. The distance actually sailed, as the author points out, is very often very different from the nominal distance, and according to the direction of the wind and the tides the relation between the actual distance sailed and the nominal distance may vary very much. Now the duration of the race takes

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account of these variations, and *prima facie* to base the time allowance on the duration instead of on the length of course would be a good way of, in effect, basing time allowance on the actual distance sailed through. On the other hand, when the duration of the race is increased, not by increase of the actual distance sailed through, but by lightness of the wind, then if the allowance is based on duration simply, you give advantage to a small yacht which she ought not to possess. A scale based on duration, but multiplied also by the wind speed, or rather by some function of it, the wind speed being measured by a wind gauge, would be a nearly perfect system of time allowance, but if the wind correction is impracticable, then the present system of basing the time scale on the distance instead of on the duration may be justified as a very fairly approximate compensation for the advantage which light winds give to small vessels.

Mr. J. Reid: My Lord and Gentlemen, I should like to make a few remarks to corroborate the views of the gentleman who has just spoken. I think he is correct in saying that vessels of large displacement carrying heavy lead keels have been found to be the best "all-round" sea boats; and I believe *that* to be the opinion of those who have had experience of racing vessels sailing round our coasts, more especially as regards small yachts, whose ballast is almost entirely on the keel. About fifteen years ago I built the 20-ton *Leander* with about 8½ tons on the keel, which was greatly in excess of any lead keel that had been previously put on any vessel of the tonnage. She was fairly successful as a racer at the time she came out, and was most successful in strong winds and heavy seas. I have since then designed, and my firm has built, a number of racing yachts, and have found that with increased displacement, and, in consequence, heavier lead keels, better sea-going qualities have resulted. It is my experience that a 5-ton racer will, as a rule, make her sea passages from port to port when old-fashioned cruisers of much larger tonnage are compelled to turn back. On looking over the names of the yachts at the head of the prize list for several years back you will invariably find that they are the vessels of the largest displacement, and, in consequence, with the additional advantage of having the greatest amount of accommodation. With reference to time allowance, it is a difficulty through which I do not see daylight; nor do I see how it is possible to scheme out a satisfactory solution of the question. The velocity of the wind is, so far as I know, an element imported into the question for the first time. If the wind blew with an equal force from the time the race began till it finished, such a scheme might be feasible; but, as this is hardly possible to expect, the theoretical advantage of the scheme therefore collapses. I think it more satisfactory to sail yachts in classes, and thus avoid the question of time allowances. The system of classification recommended by the Yacht Racing Association is the best we have yet found.

Mr. B. Martell: I did not intend to make any remarks, because this is a subject best left in my opinion to yachtsmen, but I think it necessary, after the remarks made particularly by Mr. Reid, that I should make one observation bearing on the practical nature of the subject. If it is necessary to use these heavy deep lead keels for the purpose of obtaining sufficient stability in racing, in which smallness of beam is adopted to ensure the least resistance, I would draw attention to the great importance of properly securing these lead keels to the yacht. That is a practical suggestion which, from my point of view, is of greater importance than time allowances and such matters. The importance of it is great for this

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reason, that should by any means the keel become detached the vessel would capsize immediately, and the lives of everybody on board would be imperilled if not lost. We find frequently—at least it used to be more so than now—that in order to save expense these lead keels were secured with iron bolts. Now those iron bolts in connection with lead we found to be destroyed in a very short time from galvanic action, and therefore I should like to put it forward here for the general knowledge that from my experience of these heavy lead keels, when they are used they should be secured by copper bolts. That is the practical recommendation I have to give bearing on this important point. I will not deal with the other matters because yacht owners, such as Mr. Reid, know a great deal more on the subject than I do.

The President: Gentlemen, I am quite sure you will allow me to authorise our Secretary to convey to Captain Tuxen, who is abroad in Denmark, probably engaged in important duties, because he is connected with the Admiralty there, our best thanks for the most interesting and valuable Paper which he has placed before us; and to show how thoroughly competent he is to address himself to this or almost any other subject, I may mention that you have already heard from his tutor that he passed out of Greenwich with the highest honours, and I may also add that he was the first man of his year, that is no unimportant position to hold. I am sure you will let me do what I ask you, that is, to authorise the Secretary to convey your thanks to him for his Paper.

Before we disperse I would wish to call attention to another matter. You see these most interesting engravings on the walls. They are the free gift of Admiral Paris to the Institution. They are well worth your study, and if you have the privilege of a little lecture on some of them from the Admiral himself I am sure you will all profit and be most deeply interested in everything he has to say about them. I am certain that we must all be grateful to him. He is a very old member here, and this is a proof of his friendship and the interest he takes in the proceedings of this Institution.

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MOST SUITABLE PROPELLER FOR SHALLOW DRAUGHT.

By J. I. Thornycroft, Esq., Member of Council.

[Read at the Twenty-sixth Session of the Institution of Naval Architects, 26th March, 1885; the Right Hon. the Earl of Ravensworth, President, in the Chair.]

Two years ago I had the honour of reading before this Institution a Paper on the efficiency of Guide-blade Propellers, as determined by experiments on small models.

The results obtained by some boats fitted with this kind of propeller, since built by my firm, induce me to bring the subject again before you, so as to compare the improved propeller with the paddle-wheel, its only rival in shallow water, and I wish to express my conviction that this modification of the screw will supersede the paddle-wheel in shallow water. In the same manner as the paddle-wheel has already been displaced from deep water by the simple screw, so this modification of the screw will supersede the paddle in shallow water, and from the same causes.

The paddle-wheel has been able to withstand the screw in shallow water, first, because it required less draught, and secondly, because wood or even iron vessels, of great relative length to their depth, are ill adapted to sustain the weight and vibration of a propeller at the extreme end of the hull.

The use of steel for the structure of shallow vessels has removed this last difficulty, while the first is taken away by employing the guide-blade propeller with a special form of hull, to further reduce the necessary immersion of the propeller, so the paddle-wheel has now to compete with an instrument of about the same efficiency as the simple screw, which can be successfully used in shallow water, its only fault being very limited power to propel astern, but this deficiency is balanced by an increased manœuvring power in going ahead.

If, then, I may assume that the screw, and its near relation the screw with contracting

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boss and guides, are equal, it becomes simply a question of paddle *v.* screw, and this I will now take as being more among the known quantities.

It is difficult to determine the relative efficiency of the paddle and screw with exactness. Mr. John Elder considered that a feathering paddle with thin metal floats was about as efficient as a screw propeller. Mr. Scott Russell, I think, was inclined to favour the paddle-wheel.

Mr. W. H. White, in his book on Naval Architecture, considers the screw more efficient even in smooth water than the paddle, but the *Victoria and Albert* seems to give a higher co-efficient than any screw steamer of about the same size. I think, however, the evidence so evenly balanced that for smooth water the two kinds of propeller may be considered as equal without fear of any considerable error being made if we confine our consideration to the simple question of economy of indicated power utilized in propelling. But this is not sufficient; for weight, space and cost have all to be considered.

With regard to weight of propelling machinery, I would wish to call my hearers' attention to a Table in a Paper I prepared for the Institution of Civil Engineers in 1881, on Torpedo Boats and Light Yachts.

The Table referred to gives the weight of propelling machinery per I.H.P. for a number of vessels, paddle and screw, and is adapted to show that, although the paddle engines were constructed with great economy of material for their bulk, their weight was necessarily much more than their screw rivals, and this owing to their limited power to make many revolutions in a given time. To make the comparison complete it must be stated that the power of rapid turning is enjoyed by the guide-blade propeller in the same degree as in the screw, and the extra material in the hull and propeller case, when adapted to shallow draught, is not sufficient to seriously affect the comparison.

The paddle-wheel is necessarily heavy in itself, and cannot be adapted to high-speed engines without the use of gearing, so it entails heavy engines. Its own size and weight may be explained by the small portion of the wheel acting on the propelling stream at any instant; all parts, however, which are employed necessarily have each to be sufficiently strong to resist the full force of the engines.

The continuous action of all the parts of a properly immersed screw requires no explanation, and the stress on the various parts is subject to little more than the amount of variation communicated by the shaft in the effort exerted by the engines.

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From what has already been said of the paddle, it will be evident that its great bulk as a propeller is also accompanied by a great increase in the bulk of the engines necessary to drive it, when compared with screw engines of equal power, and increased cost follows as a natural consequence, without considering the restricted carrying power of the vessel for useful purposes.

It is therefore established that the guide-blade propeller is the most suitable propeller known for shallow water, giving more available space and weight-carrying capacity in a given vessel at a reduced total cost.

To illustrate the manner in which the special form of hull used assists in producing the result described, I beg to refer to a tug boat built for the Nile and fitted with twin screws. In this boat the propellers of 36 inches diameter projected above the water line 6½ inches, but water was carried over them by the hull, and was sustained by the greater external pressure of the air, the draught of the vessel being 29½ inches, measured to the lowest part of the propellers. Careful trials were made with this boat on the Thames, December, 1875, and the experiments indicated a larger proportion of work in the tow-rope than might seem practicable. When towing at 10·7 knots, the power indicated by the engines was 93 H.P.; of this it is estimated that 37·4 were expended in propelling the boat, and from the measured stress on the tow-rope, at the speed given, it is found that 24 effective horse-power were actually expended in towing.

From this it will be seen that the efficiency of the screws in towing must be ·54 if the effective power in the shafts be taken at ·8 of the indicated work, or the combined efficiency of the engine and propellers is equal to ·43; other experiments at a higher speed gave as high a result as ·55, but are perhaps open to doubt.

Trials were also made on the Nile with the same boat in February, 1876, when a towing trial was made with a dahabeah of 48 tons displacement, and a speed through the water of nearly 9½ knots was obtained with great regularity of working.

Had this launch been fitted with the improved propellers, the draught might have been reduced to 18 inches.

The speed obtained by a shallow vessel, also built for the Nile, and fitted with one propeller only, is so satisfactory that I would ask time to give the principal dimensions of that vessel.

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	ft.	ins.
Length, extreme	56	8
Beam	7	6
Draught	1	3

Speed, 18·45 miles; load carried, 23½ cwt.

And I will conclude, by stating that an equal speed is not practicable within these dimensions using any other propeller than the one adopted.

DISCUSSION.

Mr. Arthur Rigg: My Lord and Gentlemen, I have listened with the greatest interest to the paper that Mr. Thornycroft has read about guide-blade propellers, because it was in the years 1867 and 1868 that I first brought them before the notice of this Institution. I have tried numbers of experiments, some of which have been described in the Transactions of various societies, but by far the largest number have never been published, and, as far as I have ever known or seen of what has been done by others who have tried my propellers, nothing has gone beyond those experiments which I made at Chester in 1863 and 1864,—first on a small model, which is now on the table before the Meeting, then on a canal tug boat having a 3-foot propeller, and eventually on larger steamers. In every case what Mr. Thornycroft calls a guide-blade propeller, but which I called a re-action propeller, proved most successful. There was no reason why it should not have been carried out just as easily in those days as now; but if you want to bring a really new thing into use it seems to take at least twenty years to move the inertia of ordinary minds, so far as to consider, much less to adopt, anything that goes beyond the narrow bounds of their past experiences. It seems that Mr. Thornycroft has applied these propellers of mine to small torpedo boats with great success, and there is no reason why they should not succeed, for I have applied them myself to vessels of several sizes, even up to 12 feet diameter, which does not correspond with a vessel of very small dimensions. Referring to the propeller which Mr. Thornycroft has adopted—no drawings of which appear now on these walls—I may say that I experimented with many forms of screw propellers, and the one now fixed on a small boat before the Meeting is the very first guide-propeller that was ever made or tried. There used to be a case round the screw and blades, but as the model has been lying aside for twenty years, I have been unable to find the case, although it is probably somewhere about. In another of these models you will observe a large boss, and this feature performs a very important function in all propellers. In most cases, indeed, it is absolutely necessary to have a large boss for reasons which it would take too long to go into at present. The diameter of any such boss does not depend on empirical rules, but on the progress a vessel makes per revolution of the screw, and it is a thing extremely easy to calculate. Mr. Griffiths generally made the bosses of his screws equal to one-third of their diameter, and I took the same proportion generally because it was not necessary at that time to make further investigations on this particular point. We also used guards in front of the screw we put in the tube, and did

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everything we could think of in trying all different forms, and the practical outcome of all the trials was that the propeller worked most successfully. The late Mr. Griffiths, to whom I showed the invention, acted to some extent as my coadjutor in repeating and continuing these experiments; but in the year 1877, he took out a patent quite independently of me and without my knowledge for an arrangement of leading boss to the screw and a following boss behind it, and, as far as I can understand Mr. Thornycroft's arrangement, as it is sketched on the black board, his application to the screw was much the same, except that Mr. Griffiths' boss was not quite so long as Mr. Thornycroft's. It is very likely that the longer boss will be found better adapted for very high speeds, although for the slow speeds which we had in those days it would seem reasonable to think that the length of hinder projection and the speed bear some relation to each other.

Of course I know that this Meeting does not care who is the real inventor of this system of screw propulsion, so I will say no more on that subject; but I think it will be generally allowed that the guide-blade system is the one best calculated to fulfil all the duties which any propeller has to perform, not only in small craft; but I will go further and say that any such limitation would be a very poor and imperfect appreciation of what this screw really might accomplish. In my opinion it is not only calculated to propel the smallest craft with the striking success which Mr. Thornycroft has obtained with his vessels, but it is able to propel vessels of the largest tonnage; and I venture to say, if one might prophecy, that if advantage were only taken of the increased speed at which this screw propeller can run, and if, with the present high pressures now commonly employed for steam vessels, we were to use quick running engines to suit my propellers, if this were done, then, I say that, excluding boilers, all the rest of the machinery might be made for half the present cost of an equal propelling power. Making the calculation from a commercial point of view, and having the benefit of reduced bulk occupied by machinery, we obtain increased carrying capacity; or we might use the same space and put in more power. It might possibly be considered a disadvantage that this propeller throws water backwards at such high velocity, but such an idea is founded on a misconception, because the screw is really nothing more nor less than a pump, and all you have to do is to drive as much water backwards as possible, and whether the needful resistance for propelling comes from velocity multiplied into a certain weight of water, or from weight multiplied into velocity, it comes to much the same thing in the end, and we have, in my opinion a choice as to which is the best; a choice which is comprised within very narrow limits with ordinary screw-propellers. I do not take to myself much credit for the invention of this class of propeller, as it was really a discovery made more by observations on screw propellers; then, by investigating the subject afterwards and trying some hundreds of experiments, the result has been the perfecting of the propeller which Mr. Thornycroft has brought before you. It has been known for many years, and not only has any information about it been always at the service of this Institution and the service of my country, but I would have given it with the greatest possible satisfaction; only when the Admiralty tried a number of experiments with what they know of my propeller, and Mr. Thornycroft did likewise, it seemed rather a pity that neither party thought fit to consult me, the original inventor, and so save themselves the trouble and expense of finding out for a second time facts which I had already discovered. The Admiralty must have spent a considerable sum of money experimentally with my propellor on H.M.S. *Bruiser*, but I suppose the results have never been published, and the whole affair shelved, as usual.

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What the results were I do not know, but there is no doubt if the Admiralty, before they had made these experiments, had consulted me and had given me a little information about what they were wishing to do, I could have saved them a good deal of expense and trouble in trying experiments which I had already tried myself.

This method for high speed propulsion raises a most interesting question, and I am delighted to see that it has been taken up at this Institution now. Sir E. J. Reed and many others discussed it, and took an interest in the matter eighteen years ago, but the patent ran out, and the invention has been public property, and now Mr. Thornycroft has succeeded in bringing a very good thing into notice again.

So far as the "guide-blade" propeller is concerned, it is now nothing more than what was known to the Institution many years ago, so perhaps Mr. Thornycroft will be able to tell us the results of its application to boats specially built to use it to advantage. In all hydraulics, more particularly in hydrodynamics, it is easy to theorise and consider things improvements; but when unsupported by careful experiments, such theories are nearly always wrong.

Mr. A. F. Yarrow: I think, my Lord, one of our first duties is to thank Mr. Thornycroft for having come forward to read this interesting Paper, as it will form an important record of an advance. I do not wish to go into any question as to whether the idea is Mr. Thornycroft's or Mr. Rigg's, but I think the Meeting will agree with me that, putting this point on one side, a great deal of credit is undoubtedly due to Mr. Thornycroft for having brought this system to the perfection he has. There are one or two points I should like to put to the author. He speaks of a tug boat which he built for the Nile, fitted with ordinary screw propellers, the top of the propeller being above the level of the water. I should be very glad to inquire of Mr. Thornycroft, supposing these propellers had been raised up higher, so that the top edge of the propeller was still further above the level of the water (so that the draught would have been the same as if a guide-blade propeller had been used), how the efficiency of the propellers under those conditions would compare with the propeller with which this Paper deals, because if there were no great difference in efficiency between the two, it does seem to me that the additional complication of the guide-blade would be a great point in favour of dispensing with it. I think there are many cases where this guide propeller would be quite inapplicable, and I do not look to this Meeting (as evidently Mr. Thornycroft does) as being, as it were, the funeral of paddle wheels. When we have a river to navigate, such as many on the east and west coast of Africa and also South America, rivers which frequently open out practically into marshes covered with weeds, I should like to ask Mr. Thornycroft how, in a case of that kind, he would think the weeds would affect the efficiency of the propellers. My impression is that under those conditions, which are common conditions, anything but a paddle wheel would be inapplicable. I may mention we received some years ago an order from the late General Gordon, then Governor-General of the Soudan, for four steamers for the navigation of the Nile. He gave us a draught of three feet, and we proposed at the time twin screws, but he would not admit of anything but paddle wheels on account of the weeds which existed between Kartoum and the lakes in the interior. He also objected to the use of anything but paddle wheels on account of the difficulty of navigating many of the rapids, and the known deficiency of any screw-propelled vessel to go astern instantly when required. The importance of the most perfect control over the vessel is evident from the fact that no less than four paddle steamers have been wrecked on the Nile during the last three months. To

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guide us in forming a correct opinion, I should be glad if Mr. Thornycroft would give us an idea of the probable speed in going astern with the guide-blade propeller. It appears to me that to navigate any river where there are shoals and rapids, it is a matter of the greatest possible importance to have perfect control over the boat. In a letter which Mr. Stanley wrote to me some time since referring to what was necessary for the navigation of the Congo, he laid it down as a primary condition that the boat must be under the most perfect control, so as to clear hidden rocks when comming suddenly close on them. I think you will all agree that if an ordinary screw boat is going down stream and the engines are suddenly reversed she would not answer anything like so rapidly as a paddle boat; for it must not be forgotten that the momentum of the boat itself has to be overcome, before any movement astern actually takes place; I think that consideration is a point which to a very great extent determines the nature of the propeller which should be adopted. I merely mention these points as they occur to me, and I should be very glad if Mr. Thornycroft would kindly give us his opinion upon them. There are many other considerations that I could raise, but the time is limited and therefore I will not do so, but conclude by again thanking Mr. Thornycroft for giving this Meeting the results of his experience.

Admiral J. H. Selwyn: I rise with the greatest pleasure to recognise the fact that Mr. Thornycroft gives us very short Papers, and consequently long discussions. They are very pithy Papers and very well worth discussing. He has brought before us to-night a thing which is not to be regarded as a screw at all; it is in fact a hydraulic pump overboard. It depends largely for its efficiency on the fact that it utilises the whole speed of the vessel as well as its own speed, that the water passing into the casing round the screw has a velocity to which is added the velocity of the propeller, and it becomes an extremely efficient hydraulic propeller when used in smooth water. There may be objections possibly to its use in seas where vessels would pitch in any way, but this may again be overcome. The other objection to which Mr. Thornycroft has only lately adverted, is that which we have heard before, namely, that it is extremely difficult to stop these boats. We should be prepared for that, when we consider the casing is a means not only of guiding the water into the propeller when going ahead, but is a means of preventing the water from getting into the propellers when going astern; consequently a very efficient pump when going ahead becomes a very inefficient one when going astern; but that again might be met, and I am perfectly certain Mr. Thornycroft's firm will be able to meet it. But what I wish to point out is, that we are discussing to-day a new principle of propulsion, not an improvement in screw propellers at all, which is that of hydraulic propulsion in which velocity is the main consideration; and I entirely deny and refuse to consent to the observations which were made previously, that it does not matter whether velocity or mass comes into the question. It happens that velocity gives you a value as the square of the velocity, and that the mass only gives you a value as the mass direct, and therefore it cannot be said with any justice whatever that added velocity is not required far more than added mass. If these propellers come into use for large draught vessels, as I have no doubt they will, though first fitted only for light draught vessels, they will only repeat the theory about the twin screw. Some fifteen or twenty years ago, in this Institution, being then, with Captain Symons, the only other advocate of twin screws to be found, I was told by Mr. Rennie that twin screws could only be useful in small boats. We have lived to see that prejudice overcome, and

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we now know that a principle once asserted and properly approved, is a principle good for all time; it is not a principle applicable to a model, but to all science. Nature does not deal with small models but with great facts, which are equally applicable in hydraulics and hydro-dynamics to the biggest stream human beings can by the aid of machinery possibly generate by way of producing propulsive effect or to the smallest. We are now touching the question of whether we shall continue to use an enormous size of screw with great weight and great disadvantages, or whether we can do the same work with a properly made guide-blade propeller. I think myself that the future is for the guide-blade propeller wherever it goes. To compare it with a paddle-wheel is to compare it with Noah's ark. As for the midship sections co-efficients, I have learnt to distrust these to the fullest extent, because I have seen the same vessel alter her midship section co-efficient from 200 to 2,000 by the mere altering of the screw, in spite of other additions to the resistance. It is simply absurd to talk about the midship section co-efficient with the facts we have before us. I may say that there are some other co-efficients equally liable to error. If the guide-blade propeller is to be a success, it requires velocity. Mr. Thornycroft is able to produce small engines which give high velocity, and he is able to command success in that way. I hope some of these days we shall see a fair trial of all hydraulics. I do not care what they are. It is a principle which is useful, and one as to which Mr. Gravatt's Papers, given to this Institution as a supplement to one of my feeble Papers, has been of the utmost use already, because it has disabused people's minds of the false idea that the column of pressure was the sole source of velocity. We have learnt to use "2 g" instead of "g" for gravity, and as that will go forward now, I hope in the rush for improvement which necessarily accompanies a war state, we shall see Mr. Thornycroft running boats on the Nile at the speed which he has obtained in this way. That he will eventually succeed in getting speed astern, or stoppage in some sort of way, I do not doubt. I think it might be done possibly by deflecting the current produced by this forward pump, or by some means of stoppage astern, some double rudder or something of that kind which would divert the current and reverse its action. In conclusion, allow me to say that I hope to see the whole thing make progress as a new and valuable feature in propulsion.

Mr. G. B. Rennie: My Lord, will you allow me to say one word? Admiral Selwyn has made use of my name in the discussion as to what happened some nineteen years ago with reference to a trial that was made across the Channel of two boats, one of which had paddle-wheels and the other twin screws. I perfectly remember the circumstance; it was this: the arrangement had been brought forward as rather a new way of propulsion of vessels, and at that time I alluded to several small boats we had previously made, propelled with twin or double screws, which had been very successful and which were more especially introduced for shallow draught vessels, but did not seem to me applicable for the Channel service. I do not think I made use of the expression he has attributed to me, but it had reference to the especial application of the arrangement then brought forward.

Admiral Selwyn: I think you will find it in the *Transactions*.

Mr. Rennie: As regards this special means of propulsion proposed by Mr. Thornycroft, my attention at different times has been called to it, first by some experiments, which I made on behalf of my father, of trying screw propellers used in tubes. This was a good many years ago. We found from these that

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the resistance on the screw propeller was not so great when working in a tube as when working without it, and also with rams or guide propellers in the experiments made by Mr. Rigg, and what I should like to ask Mr. Thornycroft is this, whether a great deal of this extra resistance which he gets, adding to the efficiency in the small propeller, is rather due, not to the rams or guide propellers, but to that overlapping sort of piece that he has extending beyond the screw, such as shown in the photograph, and which causes a greater pressure on the screw by preventing the water rising? I do not know whether Mr. Thornycroft has tried them separately; if so, perhaps he will kindly give us any information; what I mean is, whether he has tried the propeller with the application of the rams or guide propeller, but without that overlapping piece which he has shown in this photograph, and whether he has tried the screw propeller without the guide-blade but with that overlapping piece, and what have been the results? Mr. Rigg, who commenced this discussion, tried his propeller in a small boat that I lent him for that purpose. I witnessed the experiments, and the result was, if anything, rather to the advantage of the guide propeller, but it was not very much.

Mr. W. H. White: My Lord, I take it that Mr. Thornycroft is really giving us a supplement to the Paper that he read here two years ago; in which he described fully the principle of his propeller. He is now giving us the result of his experiments in some special vessels. Of course Mr. Thornycroft is not likely to claim any priority of conception in the matter. Not merely Mr. Rigg, but Mr. Parsons, the late Professor Rankine, and others, have had an idea of utilising by means of guide-blades some of the momentum which would otherwise have been lost in the water, to which a sternward motion had been given by the action of the propeller. But so far as I have been able to study the matter (and I have looked into all that has been done very closely) I am not aware that any one has brought out such a complete result as Mr. Thornycroft has achieved. And I am sure of this, having seen some of the experiments while in progress, that his success was only achieved by a long course of independent experiments, and by means of very numerous changes, which finally brought Mr. Thornycroft and those who were working with him to the form that he now adopts. As regards the use of this propeller in extremely shallow draught vessels, which is the subject of the Paper to-night, I think there is some force in the views which Mr. Yarrow puts forward, that the paddle-wheel may have to be used still in many special cases. It may even happen that for river navigation the paddle-wheel steamer will quite hold its own; but I understand Mr. Thornycroft to assert that taking into account the whole propelling apparatus, and the speeds obtainable in certain sizes of vessels of a certain minimum draught, he knows of nothing which approaches the performance of the small vessels that have been used on the Nile. That I understand to be the point of the Paper. It may not be possible to apply this invention in all cases, but where it can be applied it gives the best result, taking into account the weight, the space, and, I presume, the cost of all the propelling apparatus. It appears to me that Mr. Thornycroft in this guide-blade propeller of his has given a great possibility in the way of the future of steam navigation at high speeds and on our present limitations of draught. If he only goes on from these very small vessels to the consideration of the design of machinery for larger vessels of extremely great power, in which quick running engines will be used and which will not be confined to smooth water but will go over the seas, I fully anticipate that the success which he has achieved here will be extended on a much larger scale.

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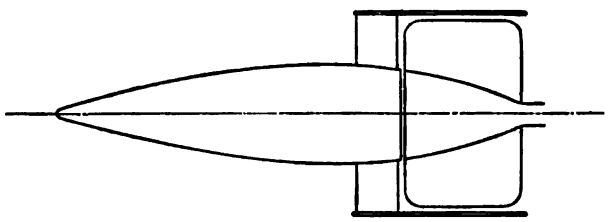
Mr. M. H. Robinson: I should like to ask one question, whether the extremely high efficiency which I understand is given with the guide-blade propeller is extended over a high range of speed, or whether it is confined to a comparatively small range of speed. That is the only question I want to ask.

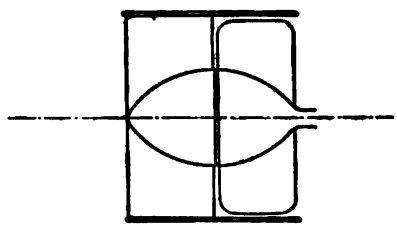
Mr. J. I. Thornycroft: I will take the speakers in the order in which they addressed the Meeting. Mr. Rigg was the first to rise, and describes the propeller I am using as *his* propeller, meaning the propeller on which he experimented, and of which I understand we have a model on the table.

Mr. Rigg: That is not correct.

Mr. Thornycroft: Mr. Rigg explained that that was the first model, and therefore I am sorry I have not the model of the propeller in the form in which Mr. Rigg ultimately made it. I understand from Mr. Rigg that he was associated with Mr. Griffiths in working this invention, and I have been unable to find many particulars of what Mr. Rigg has done, but I have found drawings of what Mr. Griffiths has done.



Thornycroft.



Rigg.

I made a drawing of Mr. Rigg's propeller, in which the boss is all contained within the tube, and Mr. Rigg asked me if I were drawing my own—from this it appears that Mr. Rigg does not appreciate the difference. In my propeller the boss is for the greater part without the tube, and the effect of the combination is entirely changed by this alteration of the form of channel within the instrument.

In Mr. Rigg's propeller the fluid entering the tube finds a contracting channel at first, and the velocity must increase as the largest portion of the boss is approached, but after this, still within in the tube, the boss diminishes in section and finally terminates, forming a channel equal to the original section of the entering stream, so the velocity again falling the fluid must leave the instrument with a velocity only equal to that at which it entered; the propelling effect is due to the velocity at which the fluid leaves the instrument, consequently the high velocity temporarily obtained in Mr. Rigg's propeller is only a source of loss. In the drawing of my propeller it will be seen that the channel for the propelling stream contracts gradually from where the propelling stream enters to where it emerges from the tube, and the result is that the greatest velocity attained is utilised in propelling; the velocity of fluid parallel to the axis of the propeller gradually increasing from the time of entry to that of discharge. This is the improvement I claim to have made, and in this the success of the instrument depends.

I was under the impression that Mr. Rigg used blades of uniform pitch, but I observe the model on the table has blades of increasing pitch and also curved guide-blades; in this the model resembles my propeller. There are, however, much too many blades, and there is no boss beyond what appears to be

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structurally necessary, and the final direction of the guide-blades is not parallel to the axis and still leaves a rotary motion in the water. That Mr. Rigg may ultimately have improved upon. The proper direction of the guide-blades at the after end is parallel to the axis.

So far as I can find, Mr. Griffiths only used guides parallel to the axis throughout their length, these could not assist in propelling but might enable the water to close in better behind the boss. It is quite possible that that was a mistake in the drawing, but what was quite distinct in this drawing was that the water got into a contracted channel and that that contracted channel afterwards expanded, as Mr. Rigg also described here, so as to reduce the velocity to that of the initial velocity when going into the tube.

Now we have to thank Mr. Rigg for what he has done in furthering the use of curved guide-blades and likewise the increasing pitch of the screw. When the screw is acting on water in a contracted channel, then the alteration of pitch is very serviceable, but when you expand the stream and again reduce the velocity you spoil the instrument.

Mr. Yarrow is building paddle steamers, and I am sorry to have said anything cruel of Mr. Yarrow's steamers, particularly when he paid so kind a tribute to my work in this Paper. Of course there may be circumstances in which a propeller placed under the vessel is not suitable. Mr. Yarrow says they will not work in weeds. Well, they are much better to work in weeds than ordinary propellers because of the means which there is of getting at the propeller. We fit in the bottom of the vessel a door for the propeller; when that door is removed the water falls to its natural level and we have part of the propeller above the water, and I must say it is the simplest thing to take any weeds and dirt out from the propeller. We have now at Chiswick a boat built by my firm drawing seven inches of water propelled by a propeller of this kind. The propeller is 12 inches in diameter, and in working in that very small boat on the river recently, the river was very dirty and we got a great quantity of sticks and dirt on. We just pulled the cover off and got the dirt out and went on as if nothing had happened. As to the power of going astern, Mr. Yarrow asks what speed can we go astern? As I have described in the Paper, the speed in going astern is very limited, but that is rather a peculiarity of the particular propeller tried than due to the principle. In a propeller fitted to Her Majesty's torpedo boat *Lightning* the propeller is differently proportioned to the one used for a shallow boat, and I have not heard of any complaint of the want of power to go astern in that boat. The manœuvring power in that boat is very great. I believe it has been described before in this Institution. The bow of that boat was fastened to the pier of Westminster Bridge and the helm was put over, the engines going at about three-quarter speed, the boat was brought broadside against the tide and she swung, against the pier, stern against the tide. I think that is a most remarkable performance for any boat. Of course, in going among rocks in difficult situations it may be found that a paddle is better than a screw propeller, but I think we must say that that requires to be proved by experiment; and if it is, as I have no doubt it is, that a higher speed is attainable with a guide propeller, other things being the same, than with a paddle-wheel, the former will be found to have the advantage,

In attempting high speed with a paddle propeller we are only courting difficulty, and the greater credit must be given to Mr. Yarrow for the results he has obtained in very shallow vessels.

Mr. Yarrow asks if the water could not be further raised by the special form of hull I have employed, so as to enable an ordinary screw to be used for shallow draught.

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This question must be divided, for, the necessary water being carried, the propeller would work without much loss if sufficient length were given to the lines leading up and down against the necessary stream, but I think it is clear, when the greatly increased weight is considered, due to the water carried and the reduction of displacement in the lines to lead up to the screw chamber, that the model referred to would be unable to bear this burden in the proposed draught of 18 inches. Some people seem to consider that if you get a high speed of piston you have a light engine. That is not so. If you double the length of your cylinder and keep your revolutions the same you get double the speed of piston in the engine and double the power, but you get probably double the weight of engine or more, so that you are exactly where you were before, and you double your speed without advantage.

Admiral Selwyn was very kind in his remarks, but I am afraid he over-estimates what can be done in high velocities. With this propeller it is practicable to use about double the slip of an ordinary propeller, and that I fully explained before. Admiral Selwyn says that in giving her velocity the thrust increases as the square. But I think to do that you must consider that you are taking more water through your instrument. Mr. Rennie wishes to know if I have tried the propeller without guide-blades in the tube, the tube projecting beyond the propeller. I must say it never occurred to me to try that, and I really do not see what more I could get out of it. One thing I did try, which, perhaps, somewhat approaches; that was a smaller screw in the tube, and as the screw was made less in diameter the water seemed to take a short cut round the ends of the blades and we got a very bad result indeed, and now we make them go as near the casing as we dare.

Mr. White says very correctly that this is a continuation of my Paper of last year, and I have to thank Mr. White very much for explaining what I am afraid I have put too shortly.

Mr. Robinson asks as to the range of speed how the efficiency varies with different speeds. Well, if you put a propeller to any particular vessel, that is a very complicated problem, because the resistance of a vessel does not vary regularly. It depends on many things, and from what Mr. Froude has shown, and Mr. Denny has shown, the resisting curve is what one might describe as lumpy. When you are propelling at points where the resistance is high, your propeller may be too small for the vessel. When the vessel runs favourably at that point your propeller may be too big; you must design your propeller so as to work best at the intended speed; you cannot vary the size of your propeller with different speeds of the boat. I have to thank you very much for the way in which you have received my Paper.

The President: Gentlemen, I am sure you will allow me to thank Mr. Thornycroft for his Paper, and I have no doubt that he is well satisfied with the discussion.

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SOME FURTHER EXPERIENCE WITH TRIPLE COMPOUND ENGINES.

By A. E. Seaton, Esq., Member.

[Read at the Twenty-sixth Session of the Institution of Naval Architects, March 26th, 1885; the Right Hon. the Earl of Ravensworth, President, in the Chair.]

My attention was first called to the modern triple compound engine by the published reports of the trial trip of the yacht *Isa*, and in it I plainly discerned the germs of a successful new type of engine; but it was not until I had seen the engines of the screw steamer *Aberdeen* erected in the workshops of Messrs. Robert Napier and Sons that I became convinced that it was the engine of the immediate future. It is, however, due to the far-sightedness and enterprise of Mr. C. H. Wilson, M.P., that I was enabled to try the merits of the new system and compare it with the old. Mr. Wilson had already viewed the triple compound engine with more than ordinary interest, and it required little persuasion on my part to allow the Company* to which I have the honour to belong to construct a triple expansion engine in lieu of the ordinary compound for one of four sister ships which it then had in hand for Messrs. Thomas Wilson, Sons and Company, the latter only stipulating that it was to be of the same power as the engine already contracted for. As I was quite convinced that economy was due to the system rather than to the higher pressure, it was decided not to increase the boiler pressure more than was necessary to suit the triple system. The other three ships already alluded to were being fitted with engines having cylinders 25 ins. and 50 ins. diameter by 45 ins. stroke, and supplied with steam of 90 lbs. pressure from a double-ended boiler 13 ft. 9 ins. diameter by 15 ft. long, having a total heating surface of 2,310 ft., so that these engines had every qualification for being economical so far as general proportions went, the stroke being an abnormally long one and the boiler of ample size. Experience has since shown that these engines are economical in coal and the wear and tear exceptionally small.

The new engines for the fourth boat were made with considerably shorter stroke, and the cylinders proportioned so as to give equal power; they are 21 ins., 32 ins. and 56 ins.

* Earle's Shipbuilding and Engineering Company, Hull.

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diameter by 36 ins. stroke, the high-pressure cylinder being supported on columns immediately over the medium cylinder, and in other respects these engines were made as near as possible like the other ones above named. Steam at 110 lbs. pressure is supplied from a double-ended boiler 12 ft. 9 ins. diameter and 15 ft. long, having a total heating surface of 2,270 square feet, and identical in design with the boiler supplied for the other engines. The propellers were made exactly alike in all respects, and the ships being likewise precisely alike, a comparison of the performances of the one fitted with the triple engines with those of the others could be made with as little grounds for differences of opinion as is possible.

One of the ships fitted with the ordinary compound engines was named the *Kovno*; that with the triple compound engines the *Draco*.

Their dimensions are as follows :—

	ft.	ins.
Length between perpendiculars	270	0
Breadth	34	0
Depth of hold	18	3

and of 1,700 tons gross register. They are ordinary cargo boats built of steel, having a raised quarter-deck and long bridge amidships, but nothing about them otherwise requires comment.

After making a voyage or two to the Baltic and finding that everything was working satisfactorily, the *Kovno* was loaded with 2,400 tons dead weight, and sailed in January, 1883, for Buenos Ayres, the *Draco* was loaded with 2,425 tons dead weight, and sailed March, 1883, for Bombay, the distance in both cases being about 6,400 miles. It was thought advisable, for purposes of comparison, that the ships should steam at as near as possible the same speed, and to attain this object we considered the surest plan was to instruct the engineers as to the average amount of coal they were to burn per day, and experience with these ships on their Baltic voyages had fixed this at 12 tons in the case of the *Kovno* and 10 tons in the case of the *Draco*. During the voyage each ship seems to have had fair average weather, and equal care was taken in getting the best results possible. The average speed of the *Draco* was, however, 8·625 knots or 207 miles per day, the engines making on the average 57·5 revolutions per minute, while the *Kovno* did only 8·1 knots or 194 miles per day, the engines making 55·5 revolutions. The coal used was ordinary South Yorkshire, just as it comes from the pits for bunker purposes. The indicated horse-power in each case would average about 600.

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The total coal consumed was 326 tons in the *Draco* and 405 tons in the *Kovno*, showing saving of 19·5 per cent. over the ordinary compounds, with an increase of speed of 6·5 per cent.

In December, 1883, one of the others, the *Grodno*, sailed for Bombay, and attained an average speed of 8·5 knots, or 204 miles per day, the engines making 57 revolutions, with a coal consumption of 12·8 tons per day, or 469 tons on the voyage. The *Draco's* consumption is therefore 30·5 per cent. less than that of the *Grodno* on the round voyage and 20·3 per cent. per day.

The success of the triple compound engine was in these instances more than had been anticipated and induced Mr. Wilson to go a step further.

The S.S. *Yeddo* had been refitted with boilers made for a working pressure of 90 lbs. per square inch, but owing to the size of the shafting the working pressure was limited to 70 lbs.; the average consumption of coal under these circumstances on two voyages was 17 tons per day. These boilers had a margin of safety beyond what was required by the rules when made, and as the Board of Trade Rules had been modified in the meanwhile, it was found that they could with safety be worked at 100 lbs. per square inch. A third cylinder was now fitted on the top of the original low-pressure, and the safety valves loaded to the 100 lbs., and the ship was despatched to Cronstadt. After making two voyages under similar circumstances to the two previous ones, the average consumption was 13·5 tons per day only. In this case it was the same ship, same boilers, same engines, same propeller and same men, the only difference being the addition of a third cylinder and the increase of pressure.

So far all the trials had been made with two crank engines, so it was now decided to construct another set of engines for 150 lbs. pressure, having a crank to each cylinder. These engines had cylinders 20½ ins., 33 ins. and 58 ins. diameter by 36 ins. stroke, and were fitted into the screw steamer *Rosario*, whose dimensions are 275 ft. 3 ins. between perpendiculars, 34 ft. 3 ins. beam, and 19 ft. 2 ins. depth of hold, 1,862 tons gross, and the dead-weight capacity 2,550 tons. In March last year she was loaded with 2,530 tons dead weight, and did the voyage to Bombay at an average speed of 8·6 knots on a consumption of 10·5 tons per day of South Yorkshire coal, and burnt on the voyage 347 tons. This result is superior to that of the *Draco* when the size of the ship is taken into account, but is not so much so as might have been anticipated from the increase of pressure and the rate of expansion, which was 14·4 in the *Rosario* and 12 in the *Draco*.

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Another set of engines was made from the patterns of those of the *Draco*, but with the high-pressure cylinder 20 ins. diameter; steam at 150 lbs. pressure being supplied from two single-ended boilers, having a total heating surface of 2,200 sq. ft. They are fitted in the S.S. *Finland*, a cargo boat, 270 ft. long, 35 ft. beam, by 18 ft. depth of hold, and 1,954 tons gross register. In January she was loaded with 2,500 tons dead weight, and sailed for Rangoon. The average speed attained was 8·42 knots per hour, or 202 miles per day on a consumption of 10·3 tons of Welsh coal per day, the rate of expansion being 12.

It should be mentioned that all these ships named are fitted and steered with steam steering gear, so that in comparing these results with those published of the engines made by an eminent engineer in the north of England, an allowance should be made, as in that ship there was no steam steering gear.

I have chosen to make all these comparisons by reference to the ships' logs, and to give results such as a shipowner looks for rather than those which engineers prefer to use in forming a judgment on the merits of different engines. I do this for two reasons: first, because the commercial success of the triple compound engines depends on the saving it can effect in a long voyage; and second, because I had no reliable indicator diagrams from which the consumption per I. H. P. could be calculated with any degree of accuracy. On trial trips with the steamers already named, the consumption of ordinary South Yorkshire coal was 1·6 lbs. per I. H. P., and the consumption of water per I. H. P., calculated from the high-pressure indicator diagrams, was 14·1 in the *Draco*, 13·2 in the *Rosario*, and 13·16 with the *Finland*, or taking the medium-pressure diagrams, it was 12·2, 13·0 and 11·95 respectively.

Twelve months ago we constructed, for Messrs. Thos. Wilson, Sons & Co., two sets of triple expansion engines of 600 I. H. P., one having two cranks and the other three cranks, the engines, boilers and propellers being otherwise exactly alike and fitted into sister ships. The water consumed in the three-crank engine is 12·93 lbs., against 13·0 in the two-crank, but the former drives its ship nearly ½ knot per hour faster than the latter does its, and when both ships are driven at the same speed the consumption of coal in the three-crank ship is considerably less than in the other.

We have now entirely given up the construction of two-crank triple expansion engines, because of the impossibility of equally dividing the work between the cranks;

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for, although the engine when running appeared to be perfectly balanced, the wear of the brasses of the crank having the two cylinders was always considerably more than that of the other. Placing the H. P. cylinder over the L. P. cylinder seemed to give the most satisfactory results, but even these were far inferior to those once obtained with the three cranks.*

We have lately constructed some very small three-crank engines from which exceedingly good results were obtained; the cylinders are only 11½ ins., 17 ins. and 30 ins. by 18 ins. stroke, which developed 218 I. H. P. with a consumption of 12·8 lbs. of water per I. H. P., and this, together with some other observations, leads me to believe that the best economical results will be obtained by running triple expansion engines at a much higher number of revolutions than is usual and with a rate of expansion not less than 12 for a steam pressure not less than 140 lbs. (155 lbs. absolute).

The largest engines we have made of this type so far are those of S.S. *Martello*, which have cylinders 31 ins., 50 ins., and 82 in. diameter by 57 ins. stroke, and indicate at sea 2,400 horse-power when running at 60 revolutions with steam of 150 lbs. pressure; the consumption of Yorkshire coal is 37 tons per day average throughout a New York voyage.

Had Welsh coal been used in every case, the results would have been very much better, for in addition to the superior evaporative power of Welsh coal, it is slow burning, and much more easily controlled, especially on the comparatively short grates of these modern boilers, the quick-burning Yorkshire coal causing the safety valves to frequently blow off when working near the load-pressure unless great care is taken by the firemen.

I trust these few particulars may be of interest to the Institution, and especially to those members of it who are particularly interested in the commercial success of our mercantile navy. I have purposely avoided engineering details and technicalities of any kind, giving only such information as will tend to give British shipowners faith in that form of engine which will undoubtedly help them to successfully tide over bad times, and keep the bulk of the carrying trade of the world in their hands.

* A diagram of the curves of twisting moments obtained with a three-cylinder triple-expansive engine will be found on Plate XII.

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DISCUSSION.

Mr. A. C. Kirk: My Lord and Gentlemen, although I have really nothing to add to Mr. Seaton's Paper, and nothing in it to criticise, still, the position I have occupied in the introduction of three cylinder engines calls upon me to say a few words. In the course of the year 1874 I first introduced a three-cylinder triple expansion engine, because I had to utilise steam of 150 lbs. in the S.S. *Propontis*. That example was followed to a small extent, but did not receive much attention. In 1876 a firm in France, at Nantes, built one; then the *Isa* was built by Messrs. Douglas & Grant, but I think I am not over-stating the case (in fact Mr. Seaton himself put it so at the beginning of his Paper) when I say that it was the construction of the engines of the *Aberdeen*, and her faultless performance on a long Australian voyage, that convinced shipowners that a triple expansion engine was in the first case a practicable machine and reliable, and in the next place a machine that would earn them money.

Now that I have so far taken credit to myself, I must also give great credit to the firm of Messrs. George Thompson & Co., for whom my firm (Messrs. R. Napier & Sons) built the *Aberdeen*. For some seven or eight years I had at different times advocated triple expansion engines with moderately high pressures, but I had never till I met with Mr. Thompson—never since the time that Mr. Dixon had the *Propontis* re-engined—got a shipowner who would believe in it, who would, as he considered, run the risk. I must say further with regard to this, that it was also fortunate that Mr. Parker, of Lloyd's, took an enlightened view of the whole subject. (I do not know if Mr. Thompson is here or not.)

Mr. C. Thompson: Yes.

Mr. Kirk: His support and knowledge of the *Propontis* assisted (perhaps considerably) Mr. Thompson in summoning the courage, and it required no small courage, to go in for the triple expansion engine. Messrs. Thompson have followed the *Aberdeen* by a similar ship with triple expansion engines, which are, in a general sense, a duplicate of those of the *Aberdeen*. In details they differ somewhat, but this is not the place to go into details, nor does the Paper lead one to do so. The only result of the difference in detail that is worth mentioning is, that in consequence the consumption of cylinder oil has been very much reduced. Now, I do not mention this for any trifling saving in the cost of cylinder oil, because that is as nothing; but I do mention it because, in a long voyage, as we do not blow off now with high pressure boilers as we used to do with low pressure ones, cylinder oil accumulates in the boilers, and sometimes forms a carbonaceous scale. Now there is no form of incrustation in a marine boiler so dangerous, or where so thin a scale (not perhaps more than 1-16th of an inch) will cause a furnace to get overheated and to become more or less collapsed, as decomposed cylinder oil. That black compound you see deposited as scale is not by any means composed entirely of charcoal. The blackness is produced by charcoal, but the greater part of it has the same composition as other scale. The introduction of this small quantity of charcoal into the scale will make 1-16th of an inch of it much more injurious than plain scale, and we have had cases in which that black compound has caused serious trouble in ordinary compound engines.

We are very much indebted to Mr. Seaton for his Paper. Although Mr. Seaton, as he says, has not had the good fortune to be able to bring forward diagrams taken during the voyage sufficiently reliable to be accepted as an evidence of the amount of the economy gained, he has brought forward other

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evidences of economy, and these taken in a rough way all round indicate an economy of 20 per cent. It is no use cavilling about 2 or 3 per cent., and I doubt whether we have in the rough actual work at sea data sufficiently correct to enable any one to do that, but I think we may safely take it in round numbers at 20 per cent. That agrees remarkably well with the results which were obtained from the *Aberdeen* and the *Australasian*, where good reliable indicator data were taken throughout the voyage, thanks to the enlightened orders that Messrs. Thompson gave their engineer, thanks also to the enlightened manner in which Mr. Mackie, their engineer, saw them carried out. Now these indicator data were not simply accepted as they come from the engineer without analysing them. As they came from the engineer we had the diagrams, we had the revolutions taken by a watch at the time the diagrams were taken, and we had daily the revolutions by the counter. The revolutions by the indicator were taken as giving the true average, and the horse-power was corrected in the proportion the cubes of the revolutions at the time the diagram was taken bore to the mean revolutions taken during the day as obtained by the counter. In that way an average power was obtained. The average consumption on the voyage was thus found to be 1·6 lbs. per indicated horse-power as correctly as can be. Now, that is just 20 per cent. under two pounds, and I am not aware of any ordinary compound engine, treated in the same rigorous manner, that would burn less. I can quite endorse all that Mr. Seaton says, that the form of triple expansion engine with three cylinders and three cranks is in every way the preferable arrangement. I did, indeed, patent an engine with four cylinders and two cranks, which gave very equal strains, and in some cases it might be convenient, though I, myself, have never used it. But the three cylinders and the three cranks, besides the advantage that the Marine Superintendent will appreciate of being rapidly and conveniently overhauled, has the advantage common to all three cylinder engines that you have a very much more uniform tortional moment on the shaft and propeller. In two cylinder engines the greatest and least stress will often vary as much as three to one, but in three cylinder engines it is very much more uniform. I remember many years ago, my friend Mr. Macfarlane Gray, who is now sitting before me, remarked on that very subject, that an intermittent action of the propeller was very wasteful, because these sudden short propulsions given to the water went very nearly all into slip, and very little into propulsion. What Mr. Seaton has stated bears that out. I am not able myself to put a correct value on it, but I have had through my hands a great number of three cylinder engines, and I have certainly observed that the performance in them, as far as the propulsion of the ship was concerned, was superior to that of two cylinder engines. The number of these engines now made is very large, in fact, astonishingly large, considering the very short period during which they have been prominently before the engineering world. A list was published in *Engineering* lately which quite astonished me. I will not read it now, because any one can find it for himself. My own firm have made 22,500 indicated horse-power of these engines, which is, as was natural, perhaps, much the highest that have been done yet. With regard to the triple expansion engine, speaking of it as an engine proper, Mr. Thompson's experience, my own observation, and Mr. Seaton's observation, all agree that the wear and tear is not more than in an ordinary two cylinder engine, as indeed it ought not to be. The strains are not more severe nor more irregular in any way, and the higher temperature in the triple expansion system has not had any prejudicial effect. But the engine is not the only thing, we have got the boiler also. The boilers of the *Aberdeen* have answered uncommonly well, and so have those of the *Australasian*. I could quote a case where the boilers have given trouble, arising entirely from scale and

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deposit, but that is not to be at all attributed to their being triple expansion engines, or to their having a somewhat high pressure. The result in that case would have been the same whatever pressure they had been worked at. But I will not go any further into this, seeing that the subject of boilers is coming before us to-morrow evening.

Mr. W. Parker: My Lord, Mr. Kirk having mentioned my name in connection with the introduction of this triple expansion type of engine, I feel I should say a few words. Some three years ago I read a Paper at the Meetings of this Institution on "The Economy of Compound Engines," in which I endeavoured to show that, in my opinion, no great economy of fuel can be realised by increasing the steam pressure above 80 or 90 lbs. per square inch, and using the steam in the ordinary two-cylinder type of compound engines. That Paper led to considerable discussion. I think, however, the results that have been obtained from the introduction of the triple expansive engines, particulars of which have been given by Mr. Seaton to-night, have somewhat proved the views I then had and the statements made. I may say that from the time when these engines were first introduced by my friend Mr. Kirk, now some ten or eleven years ago—I refer to the engines of the *Propontis*—I have been in my official position intimately associated with them. When that vessel was completed, Mr. Kirk, Mr. Brock (of the firm of Messrs. Denny and Co.) and myself conducted a series of trials at sea over an extended period with these engines, which satisfied us that if pressures were to be increased a different engine from the compound engine must be resorted to. Steel was not introduced at that time—there was nothing but the old iron boiler, and we could not with the thickest plate that could be manipulated then obtain a higher pressure than about 70 or 80 lbs. per square inch, so that the thing, as Mr. Kirk has represented, remained at a standstill until the introduction of steel, and until he could persuade some enterprising shipowners, like Messrs. Thompson and Company, to take advantage of it. With regard to Mr. Seaton's Paper, I think, my Lord, we must all feel indebted to him for giving us the results of the performances of these ships. I can in every way corroborate those results, and perhaps it might be interesting, as coming from one having nothing whatever to do—I mean from a monetary point of view—with the making of engines or boilers, or the ownership of ships, to state what has come under my own observation with regard to the performances of some of these engines. I took particulars of two vessels built on exactly the same lines, from the same model, the same power put into both of them, the same boilers—I mean as far as the generating power of the boiler is concerned—and the same propeller. The vessels were 2,450 tons gross register. The first vessel was fitted with an ordinary compound engine to work at a pressure of 80 lbs. per square inch, and she steamed from this country on a voyage of some fifteen days out and some fifteen or sixteen days back again. Her consumption was 13 tons per day, and her average speed during that time was 8½ knots. Now, the sister vessel, which was building at the same time, was fitted with a triple expansion engine to work at 150 lbs. instead of 80 lbs. She sailed on exactly the same voyage, at the same draft, and, practically speaking, experienced the same sort of weather, there being no bad weather in either voyage; she steamed 9 knots per hour on a consumption of 9½ tons per day. These figures are not taken from interested parties, or from what this or the other one has said, but they are taken from the engineer's log, the captain's log and the owners' own books, the persons who had to pay for the coals consumed, so that there can be very little doubt about their accuracy. With regard to the wear and tear in these engines, I have taken a great deal of pains and trouble to inspect the machinery of some of

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these vessels on their return from various voyages, especially the S.S. *Aberdeen*. I have examined her boilers internally and externally, and also her engines, every time she has returned to this country, and from my observations I can safely say there is not the slightest difference in the wear and tear of a triple expansive engine as compared with the wear and tear in an ordinary compound engine. But when we come to the treatment, that is somewhat another matter, so far as the boilers are concerned; they require to be treated more carefully, I mean with regard to the feeding, blowing off, density, &c., especially when engaged on long voyages. At the same time, I think it is only just to the engineers of the *Aberdeen*, who managed her so well, to state that her boilers have not suffered at all—in fact, she is now, I think, on her ninth voyage round the world, and with the exception of renewing baffle plates, that have been burnt off, fire bars, and such things that cannot be helped, these boilers have had no repairs done to them. But Mr. Kirk has referred to other matters which would cause these boilers to give trouble, and which at the same time do not affect the principle of the triple expansion engine at all —I refer more particularly to the residue from the mineral oil used in lubricating the cylinders, piston rods, &c. This oil, passing as it does through the cylinders and condenser into the boilers continuously for a period of, say from 35 to 40 days, leaves a residue in the boiler of such a nature that it is the worst non-conductor of heat that I am acquainted with, and when in contact with the furnaces admits of the plates becoming over-heated. To illustrate its non-conductive properties, I took from some boilers, where a great amount of this oil had been used, and the furnaces had collapsed in consequence, a quantity of this residue, which had the appearance of treacle, and experimented with it. An ordinary bucket was half-filled with water and placed on an ordinary smith's fire, and the water allowed to boil. Then the bucket was lifted from the fire, the bottom being so cool that you could put your hand under it, and a solution of this residue was painted on the bottom of the bucket and the experiment repeated. The bottom was found, on raising the bucket, to be red hot. If that sort of thing goes on in a boiler you must necessarily expect to have furnaces collapsing and coming down. My principal object in again mentioning this experiment before this Meeting is, that a great many complaints have been made to the effect that boilers are suffering very much more than they used to do, owing to the adoption of higher pressure and these triple expansion engines, &c., &c. I mention this to show that the partial collapsing of a furnace has nothing whatever to do with the principle of the engine or increased pressure, but with the management of the boilers, and in order to avoid this, those who manage these high-pressure boilers, especially those engaged on long voyages, will have to be more careful and exercise more intelligence in their treatment. To show how this engine is progressing, his Lordship mentioned some figures in his address the other day, and I may state that now there are some 36 or 38 steamers afloat fitted with them, and the owners are perfectly satisfied that they have a considerable saving of fuel by its adoption, and to still further prove its advancement there are at this time no less than between 50 and 60 pairs making.

Mr. W. Denny: My Lord, I regret that the absence of my partner, Mr. Brock, obliges me to take up a subject to some extent foreign to my ordinary course of work; but when such a question as the triple expansion engine is discussed, all those firms who have had experience with that engine should do their utmost to contribute to the discussion. We have now two sets of these engines running at sea, and we have found in such regular work, as well as in experiments which we made upon a small screw steam

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tender of our own, very much the same results in economy as Mr. Seaton has described, viz.: that the consumption per indicated horse-power was as nearly as possible 1·6 pound, and the economy about 20 per cent. Mr. Seaton has evinced a strong objection to the two crank triple expansion engine, but I think his objection will be found only to apply to the kind of two crank triple expansion engine with which he himself has worked, I mean the two crank engine in which there are two cylinders over the one crank, and one cylinder over the other. The engines which my firm now have afloat are of the two crank description, but have four cylinders, the high pressure and one low pressure cylinder being over the first crank, and the intermediate and the other low pressure cylinder over the second crank. With these engines we have had in every way excellent results in economy and also in steadiness of working. We have, at present, three other sets of them in hand, besides two sets of three cylinder three crank engines, but as they are not working I cannot speak about them. There is a good reason for drawing your attention very closely to the four cylinder triple expansion engine, because it is the same in form as the engine which, I believe, will supersede the triple expansion, that is the quadruple expansion engine. We have at present working in our steam tender a quadruple expansion engine, and from experiments which we have made most carefully, comparing it with the compound and also with the triple expansion engine, we find that a very decided economy can be gained over the triple expansion engine, not certainly so large as the triple expansion engine has effected over the compound, but still an economy sufficiently large to be worth the attention both of engineers and ship owners. This two crank quadruple expansion engine having two cylinders over each crank, and having all four cylinders of different sizes, which my partner, Mr. Brock, has designed and introduced, has the further advantage, that it is the very best arrangement by which the present compound engine can be converted into a more economical type. When we speak of these new and more economical engines, it is evident the subject has a very decided interest for the naval architect. It opens up to him great possibilities, not only in the way of performing economically long voyages which were before almost impossible, but also in the way of carrying out express services. I believe such engines will enable the performances now being realised on the Atlantic to be very largely surpassed, without any increased expense in coal. If any of you, gentlemen, have made the attempt to design an express steamer for speeds over 20 knots at sea, you must have found that one of the greatest difficulties is to stow the boilers on board. But a saving of 20 to 30 per cent. in consumption means a saving of 20 to 30 per cent. in the boilers required on board, and that, taken together with the saving in the coal space, very greatly facilitates the construction and designing of high speed express service steamers. I need not refer in the presence of so many gentlemen interested in the construction of war ships, to the possibilities these engines put into their hands. With the demand in our war ships for increased weight of armour and for greater coal endurance, there is no question but that these new engines, whether quickly or tardily adopted, offer to the designer of war ships the greatest facility which he has had within the last ten or twelve years.

Mr. Cornelius Thompson: My Lord, I have had the honour of being a member of this Institution for some eight or nine years, but I have never yet had the temerity to address it, and therefore I hope you will excuse me if I fail at once to command the attention of the Meeting. I would not venture to speak on the subject, but that I have some practical experience of results of one of the vessels referred to by Mr. Seaton in his Paper, and more particularly by Mr. Kirk and Mr. Parker. They have referred to

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the working of the steam ship *Aberdeen*, and Mr. Denny very properly alluded this afternoon to the advantage of having practice as well as theory. Now, perhaps Mr. Kirk and Mr. Parker may be looked upon as theoretical men with regard to the consumption of a steam vessel on a long voyage, but the owner you may depend upon it is a very practical man, and he knows perfectly well the amount of his coal bill, and no amount of glossing or theorising will get over the amount of the cheque that he has to hand over. The *Aberdeen* has now run some 170,000 miles and I am very glad to be able to corroborate the remarks of Mr. Parker and Mr. Kirk. The wear and tear has been no more than would be found in any ordinary vessel, and there has been no cutting or cording in the face of the high pressure cylinder or valves. And with regard to the boilers, which are a ticklish point, so far as I know, there has not been a new rivet put into the boilers of the *Aberdeen*. I think, gentlemen, you will agree with me that this is a most satisfactory result. There is only one other remark I would make with regard to the sister ship, the *Australasian*. On this last voyage from Plymouth to Adelaide the engines were never even slowed, they were pounded along for 40 days without being once stopped, and the consumption from an owner's point of view is 1·6. I should add, I think, that the diagrams were taken carefully every second day by the chief engineer.

Mr. G. Rodger: My Lord, I feel very great diffidence in coming after the speakers who have preceded me, but there are one or two points on which I feel it my duty to speak. One that I specially noted on my paper was Mr. Seaton's remark that they had found it impossible to construct two crank engines on account of the great irregularity of the strains; well, the firm with which I am connected has built already a two crank engine in which the strains are as equal as in any engine (not a three crank engine), they can possibly be. The arrangement is no doubt known to many in this room, and consists of two high pressure cylinders placed tandem fashion over an intermediate and a low pressure cylinder, so that the piston rods being produced through the intermediate and the low pressure covers, are fastened to the two high pressure pistons. In the ships of which I am speaking, there are two 17 in. cylinders, worked with a pressure of 150 lbs., the intermediate cylinder being 38 in., and the low pressure 60 in. diameter, all having a stroke of 3 feet 6 inches. The steamer to which this engine was fitted has now made two complete South American voyages (to the Amazon and back), and has given to her owners and engineers, and indeed everybody concerned, the most perfect satisfaction. It is a very great pleasure to me to endorse in a small way what Mr. Seaton has said about the economy, and about the small amount of wear and tear that has been found with these engines. I believe that they had not even broken a gauge glass on the boiler. The packing has not given the slightest trouble, and in every way the engines have been most satisfactory. If I am in order, I should like to say a word about the quadruple expansion engine, which Mr. Denny has touched upon. Such an engine was designed at the works at Barrow, and when it was about half made we found that Mr. Brock was a prior inventor. He was about three weeks or so before us with the idea, so far as we could learn, but the engines were by that time half made and are now at sea. We have not received particulars sufficient to enable us to form an exact judgment as to economy, but the result of a voyage to New Orleans shows that they are at least as economical as the best type of triple expansion engines. The vessel is now on a Rangoon voyage, and on her return, no doubt, we shall have results as to economy and other information, which shall be heartily at the service of the Institution. Before I sit down, I would say that at present we have on our books six sets of triple expansion engines of from 760 to 6,000 horse-power.

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Mr. A. E. Seaton: My Lord, I am very pleased that this short Paper of mine has been received with interest by the members of this important Institution. It was only at the last moment that I was asked to contribute a Paper, and my first impression was to give it from an engineer's point of view, but on second thoughts, I came to the conclusion that, seeing the interest that was taken in the question generally, it would be better to take it from the broader point of view enjoyed by the shipowners; and as I have said in my Paper, I kept as clear as possible from technicalities, and gave only the coal consumption as recorded in the shipowner's books. I may mention, however, that the *Martello*, the ship which I have alluded to shortly, is a large Atlantic steamer, having a much larger staff of engineers than the other ones: they have been able to keep a record of the I.H.P. throughout the three New York voyages which she has accomplished without a single hitch; the consumption has been only 1·5 lb. of Yorkshire coal per I.H.P., for the I.H.P. has ranged from 2,300 to 2,400, and the consumption on the average was 37 tons a day. With regard to the boilers, having now made 11 sets of these engines, and having others in hand at the present time, I can speak now with some little confidence about the matter. Very little trouble has been experienced, in fact no more trouble than was experienced with the old 60 lb. boilers, when they were first made; I may almost say less trouble. The chief trouble has arisen when cooling down, on the ship coming into port. When at sea, working with full pressure, there seems to be no trouble whatever, but in one or two of the ships, engineers have reported that in cooling down, after arriving in port, they have had a considerable amount of leakage; the causes of which we are investigating at this time, and therefore I will not go into that now. There was another thing which gave us a little trouble at first, viz.: the jointing of the mountings at the boiler. We had more trouble then with the 110 lb. pressure, than since with the 150 lb. pressure, because we found out the cause, and conquered it. I can quite corroborate what Mr. Parker has said, respecting the economy being due to the system, rather than to the pressure, for what set me thinking several years ago about this system, was the fact that a boat in which I took some interest seemed to do better when her boiler pressure had been cut down by the Board of Trade from 70 to 60 lbs., than she did originally with 70 lbs. Her speed was rather better than it had been before, while her consumption of coal was no more, and the wear and tear of the engines was considerably less. We have experienced no trouble, so far, with any of the boats from the oil. I think that arose in a great measure from the engines being fitted with what is known as the visible supply lubricator, in which the engineer can see every drop of oil which passes into the cylinders. Since ships have been so fitted, there has been a very large saving of cylinder oil, and the regularity of supply is everything that can be desired. The consequence is we find that the deposit of oil, or rather refuse, in the hot wells and boilers, is nothing to what it used to be, and in these high pressure boilers we have seen no indication of any injurious deposit at all. I must say that I cannot agree with either Mr. Denny or Mr. Rodger, as to the two crank engine. I have tried both in every possible way, and as I have told you, at whatever cost, we preferred to make three crank engines. There is no question as to wear and tear, for whatever the best two crank engine may show, as regards smallness of wear and tear, the three crank engine will beat it. I can say that without fear of contradiction, because I have a very good basis on which to make the statement. There is another point which has not been mentioned, to which I should wish to draw your attention. The three crank engine can be driven at so slow a speed, as to keep up steerage way with very little head way; this I think, is a very great desideratum when going in or out of dock, or when the ship is wanted to be moved

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at an exceedingly slow speed, more for the sake of steering than anything else; that has been effected with the three crank engine in a far better way, so far as I have seen it, than with any two crank engine. Then again, when you take into account the necessity of a Captain keeping his ship under command in a fog, this is a very considerable advantage. Mr. Denny has mentioned about the express service. I may say, that at the present time, we have in hand a very fast boat for express passenger service, of a speed and size that is not possible, except by using the triple compound engine, and that entirely owing to the smallness of the boilers and bunker space. This is comparatively a small boat, having to make a moderately long voyage at very high speed. We attempted the same design four years ago, and found it was utterly impossible. This ship will be launched next week, and will be taken on a trial trip in six or seven weeks, when we hope to obtain from her some very satisfactory results. I may mention that some little trouble has been experienced with the gauge glasses, which seem to dissolve away in a marvellous manner, that I can hardly understand. It seems as if they were easily dissolvable in water. We are trying to persuade the glass gauge makers to get a better kind of glass for gauges, and in that way I hope we shall be able to conquer that, as we have conquered so far every other difficulty.

The President: I need not say I know that I may return our very best thanks to Mr. Seaton for this most valuable Paper. I cannot conceive a Paper more likely to be of interest and of value—real value received—to the shipowners of this country, than the Paper we owe to Mr. Seaton to-night. Allusion was made by Mr. Denny not long ago, as to his opinion with regard to the applicability of the triple expansion engine to war ships. It may be of interest possibly to some of you to know that there is building in this country, at this moment, a war ship which is going to be fitted with a triple expansion engine.

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THE THEORETICAL DUTY OF HEAT IN THE STEAM ENGINE.

By J. MacFarlane Gray, Esq., Member of Council.

[Read at the Twenty-sixth Session of the Institution of Naval Architects, March 26th, 1885; the Right Hon. The Earl of Ravensworth, President, in the Chair.]

It was before the Institution of Naval Architects, in 1880, that I first gave publicity to the numerical results I had obtained by working at steam on the lines of the ether-pressure theory which I then explained.

Up to the date of that Meeting I had been satisfied with the fair degree of approximation which is obtained when, geometrically, the differences are within the ordinary thickness of the lines of a diagram of convenient size. The next day after reading the Paper, I began to see that I had in this investigation something far more valuable than I had before imagined it to be, and that I ought to push it forward with all the strictness of accuracy which seven-place logarithms would admit of and that the varying specific heat of water which had always been neglected in the investigations of others, really plays an important part in the geometry of thermodynamics, and that, therefore, these varying rates had to be ascertained more correctly than they were then. A difference of the thickness of a line at the boiling point on the vapour-gas line H_0H, on the temperature-entropy diagrams was discovered, and that could be rectified only by a very much greater shift in the entropy of the state-points for water. How that was to be done, and the known specific heats of water at different temperatures also accurately taken into account, I could not at that time determine, because I had also to conform the alteration to the "second law" of thermodynamics. Now, according to the "second law" the state-points for water must lie on the initial boundary of the thermodynamic field A_0A, a line which is fully known when the vapour-pressures for different temperatures are known. These are known for steam, and the state-points for water could not, therefore, be shifted in entropy without conflicting with the second law. I had never any doubt as to the soundness of the geometrical treatment of thermodynamics which I introduced in my paper in 1880, but I saw that a definite step forward had to be taken

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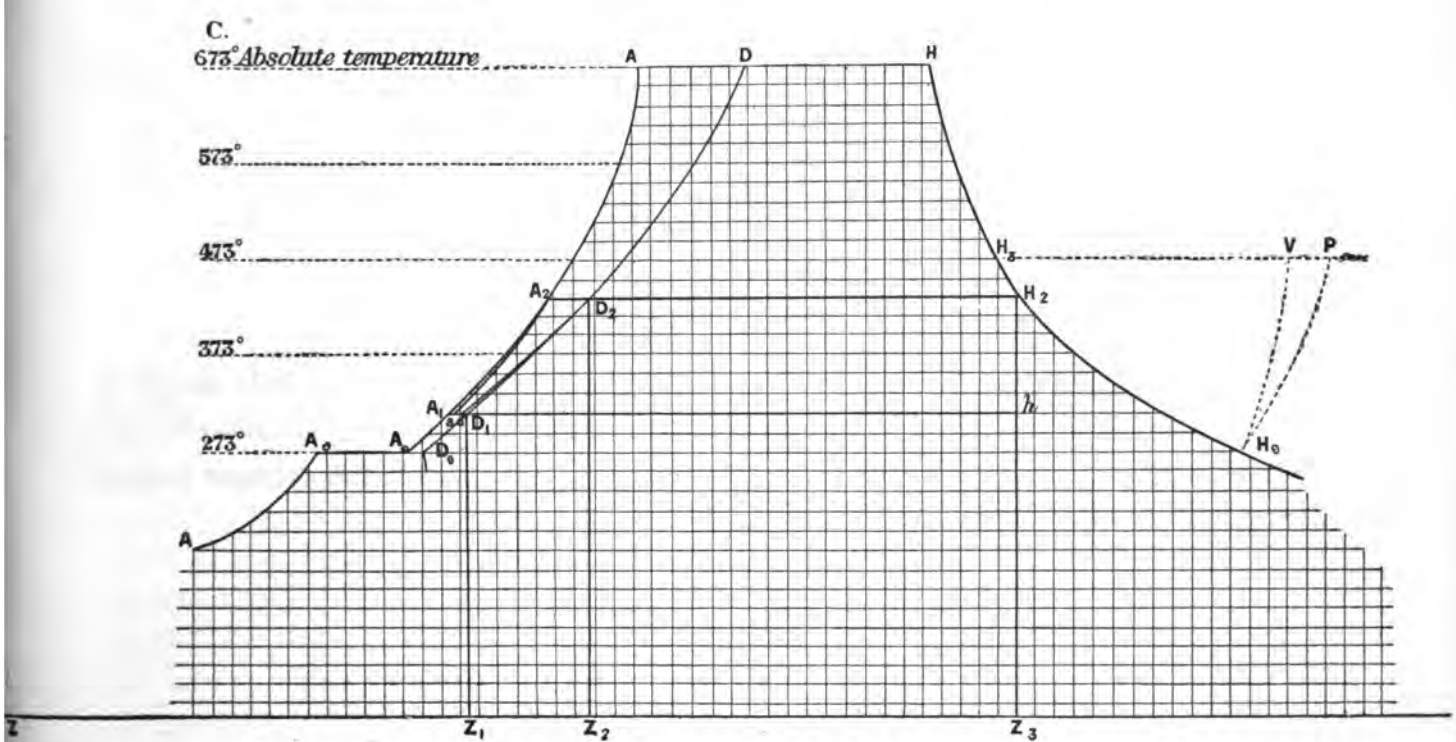
beyond my Paper, and I thought it my duty to the Institution and to myself to hold back the Paper from the *Transactions* until that step had been made. I explain to the members of the Institution that it was solely at my request and contrary to the wish of the Council that the Paper was not included in the *Transactions;* the printer's account, £15. 2*s*., was paid by me rather than have in your *Transactions* a statement requiring important modification. I trust that my example will be followed in all similar cases.

I have found it not an easy matter to make the alteration required. It would be an easier task to move London Bridge a few feet up the river than to move the vapour-gas state-points on my diagram the thickness of a line in entropy. I have been working at it for five years since the diagram was exhibited here, and I have not moved them a hair's-breadth. The "second law" said I must not shift the water state-points, and the vapour-gas state-points would not move, and in this dilemma I have appealed to a higher court and have obtained a judgment setting aside the "second law."



THETAPHI OR TEMPERATURE-ENTROPY DIAGRAM.

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The decision is given in this form: Irrespective altogether of any new theory of mine the position and slope of the initial boundary, A_0A in fig. 1, of the thermodynamic field of the temperature-entropy diagram of the step, evaporation, is defined by the series of temperature-pressures of the substance; if, therefore, the water state-points lie on that boundary, the varying specific heat of the liquid must conform to the initial boundary by the temperature-pressures determined. That is to say—

$$\mu w - \frac{\gamma}{\gamma - 1} = \begin{cases} \epsilon^2 b \log. a\, a^x (2\,\theta + \theta^2 \epsilon \log. a) \\ + \epsilon^2 c \log. \beta . \beta^x (2\,\theta + \theta^2 \epsilon \log. \beta) \end{cases} \qquad (1)$$

according to Regnault's formula (H) for temperature-pressures for steam. Here the letter μ stands for the molecular weight of the substance relatively to that of hydrogen; w is the specific heat of water at the absolute temperature θ, and γ is the ratio of the specific heat of the gas of the substance at constant pressure to that at constant volume. The letter ϵ is the ratio of the napierian to the common logarithms, viz., 2·30258509, and the other letters have the following numerical values in Regnault's empiric formula (H).

$$\log. F = a - ba^x - c\beta^x \qquad (2)$$

F = pressure in millimetres of mercury

$\log. a = \bar{1}{\cdot}994049292$

$\log. \beta = \bar{1}{\cdot}998343862$

$\log. b = 0{\cdot}1397743$

$\log. c = 0{\cdot}6924351$

$a = 6{\cdot}2640348$

$x = 20^\circ +$ the temperature centigrade.

I have worked out the values of the above expression for the whole range of temperatures to which Regnault's formula (H) applies. I have also done the same for the similar formulæ (D) and (F). These values for $\mu w - \frac{\gamma}{\gamma - 1}$ are given in the columns headed D & F, and H respectively in the following Table :—

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1 T.	2 θ	3 D. & F.	4 H.	5 Reg. & Cl.	6 Specific heat, "Second Law."	7 Specific heat, Regnault.	8 Specific Heat, Ether pressure Theory.
0	273	0·2844	5·2360	4·6516	1·0649	1·0000	·9989
10	283	1·4367	5·2824	4·6561	1·0701	1·0005	
20	293	2·4288	5·2506	4·6624	1·0666	1·0012	1·0042
30	303	3·2519	5·1598	4·6696	1·0565	1·0020	
40	313	3·8690	5·0210	4·6786	1·0410	1·0030	1·0098
50	323	D 4·3484	4·8521	4·6894	1·0223	1·0042	
60	333	4·5939	4·6623	4·7020	1·0012	1·0056	1·0157
70	343	4·6128	4·4611	4·7164	·9788	1·0072	
80	353	4·3810	4·2565	4·7317	·9561	1·0089	1·0220
90	363	3·8441	4·0833	4·7497	·9369	1·0109	
100	373	3·0365	3·8598	4·7686	·9120	1·0130	1·0286
100	373	2·2324	3·8598	4·7686	·9120	1·0130	
110	383	2·7223	3·6766	4·7883	·8917	1·0153	
120	393	3·1204	3·5076	4·8109	·8729	1·0177	1·0357
130	403	3·4175	3·3548	4·8342	·8559	1·0204	
140	413	3·6033	3·2197	4·8604	·8409	1·0232	1·0434
150	423	3·6587	3·1029	4·8874	·8279	1·0262	
160	433	F 3·5825	3·0124	4·9162	·8179	1·0294	1·0516
170	443	3·4376	2·9254	4·9468	·8082	1·0328	
180	453	2·9028	2·8515	4·9792	·8000	1·0364	1·0604
190	463	2·2454	2·8188	5·0125	·7964	1·0401	
200	473	1·3246	2·7923	5·0476	·793	1·0440	1·0700
210	483	0·0899	2·7793	5·0845	·7920	1·0481	
220	493	—1·5205	2·7826	5·1232	·7923	1·0524	1·0804
230	503	—3·5836	2·7981	5·1628	·7941	1·0568	

Regnault points out that the formula (F) does not reproduce in a very exact manner the numerical data employed in its construction, owing to the circumstance that the sum and the product of the two roots of a quadratic equation used in the construction of the formula are both very small fractional numbers, and the use of logarithms with seven places of decimals did not permit of the calculations being made sufficiently rigorous. The formula (D) has been criticised by M. Moritz in the "Bulletin Physicomathématique de l'Académie des sciences de St. Pétersbourg," t. II. (1854-1856). He had detected an error in its construction; his amendment is adopted in the "Principes de Thermodynamique," by M. Paul de Saint-Robert and by Clausius. These remarks virtually annul any value which these formulae (D) and (F) might otherwise have been thought to

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possess. To the formula (H) there has never been any exception taken, it is that which gives Regnault's pressure tables and curves. I have given also the value of $\left(\mu w - \frac{\gamma}{\gamma - 1}\right)$ according to the specific heats of water for the different temperatures according to Regnault's formula, and according to the value of γ for steam used by Clausius, viz., $\frac{\cdot 4805}{\cdot 370}$, in the column headed "Reg. and Cl." In the 6th column, under the heading "Specific Heat—Second Law," I have given what the specific heat of water must be at the given temperatures if the experiments of Regnault are reliable, according to formula (H), and according to the value of gamma given above, on the assumption that the first law of thermodynamics is true, that the γ ratio is constant for perfect gas, and that the *splitting-heat* or energy of segregation *per se* of liquid into gas is a constant for water at all temperatures; these being generally accepted principles in standard works on thermodynamics. The experiments on the specific heat of water at different temperatures are not quite satisfactory as published by Regnault, but they are certainly not so wide of the mark as that column of the preceding Table would show them to be. In Poggendorff's *Annalen*, 1874, M. Bosscha has shown that the specific heats of water, as given by Regnault, are probably 1½ per cent. less than they ought to be, according to the experiments. The 7th column gives the specific heat according to Regnault. It is evident from a comparison of columns 6 and 7, that they are not at all in agreement. On the other hand, when the water state-points are placed where the ether-pressure theory requires them to be, viz., on D_0D, the specific heat is almost identically that deduced by me on other grounds from the first law of thermodynamics in a Paper read before the Physical Society of London in November, 1880. The specific heats according to that theory are given in column 8. According to that Paper the specific heat of liquids consist of two parts, the ergonal and the thermal constituent of the quantity called the specific heat. The thermal constituent is constant for the same substance at all temperatures, it is—

$$k = \frac{8 \cdot 5}{\mu} \qquad (3)$$

The ergonal constituent of the specific heat of liquids is—

$$x = \frac{8 \cdot 5}{\lambda_1 \mu^2}. \qquad (4)$$

where λ_1 is the inner heat of evaporation, that is, the heat of evaporation at the temperature for which x is required, less the thermal equivalent of the $p\,v$ of the vapour. This is A D in the figure. For 8·5 read, more accurately, 8·50648.

In forming these expressions, I have in the meantime had to neglect the variation of volume of the liquid at the different temperatures. I shall return to that when I get reliable

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data to work from, and the above constant may then have to be slightly modified. As the expressions stand they are very much nearer the truth than any empiric formula now in use.

Returning to formula (1) I explain that when the ether-pressure theory is followed I get for the value of the expression on the right, for which I now put the letter R—

$$\text{not } R = \mu w - \frac{\gamma}{\gamma - 1}. \qquad (5)$$

$$\text{but } R = \left(k - \theta \frac{d w}{d \theta}\right) - \frac{\gamma}{\gamma - 1}. \qquad (6)$$

and as this brings the liquid state-points within the thermodynamic field, by entropy $= x$, it follows that instead of the "second law" of thermodynamics which is—

$$E = \frac{\delta \theta}{\theta} \qquad (7)$$

we get for the limiting efficiency or duty of heat in a perfect engine—

$$E = \left(1 + \frac{8{\cdot}5\ \theta}{\lambda \lambda_1 \mu^2}\right) \frac{\delta \theta}{\theta}. \qquad (8)$$

Here, λ is the heat of evaporation at the absolute temperature θ, that is what is commonly called the latent heat of evaporation at the given temperature, and λ_1 is the same as λ but the thermal equivalent of the $p\ v$ of the vapour is deducted from λ to get λ_1; as before μ is the molecular weight of the substance in relation to that of hydrogen. On the diagram $\frac{A_2H_2}{D_2H_2}$ = the value of the co-efficient of $\frac{\delta \theta}{\theta}$ in (8), for temperature 160° C.

Formulæ (3), &c., apply to any thermometric scale. The constants in formulæ (1) and (2) are, of course, for the centigrade scale only.

I will now give the ratio of the respective values of E by formula (7) and by formula (8) for the practical range of temperature in the modern steam engine, using throughout the values for λ, λ_1, obtained on the ether-pressure theory.

Temperatures.		Co-efficient of $\frac{\delta \theta}{\theta}$ in formula (8).	Pressure, pounds per sq. inch.
Fahrenheit.	Centigrade.		
212°	100°	1·05780	14·7
284	140	1·07951	52·6
356	180	1·10841	146·0
428	220	1·14729	336·3

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That is to say, the efficiency of the steam engine is, theoretically, nearly 6 per cent. greater at 212° F. than the "second law" makes it, and at 428° F. it is nearly 15 per cent. greater. This refers only to the reversible cycle change-of-state engine.

I give the following table of pressures to show the kind of agreement there is between the ether-pressure theory and Regnault's experiments :—

PRESSURE IN MILLIMETRES OF MERCURY.

Temperature.	Regnault.	Ether-Pressure Theory.
—20° C.	·927	·939
0	4·60	4·549
20	17·39	17·372
40	54·906	54·868
60	148·791	148·515
80	354·640	354·125
100	760·	760·
120	1491·28	1493·332
140	2717·63	2723·410
160	4651·62	4661·429
180	7545·00	7556·910
200	11688·96	11692·111
220	17390·	17373·932
230	20915·	20882·370

Referring to the diagram, D_1 is the state-point for water at 40° C. At D_2 it is water at 160° C. and the heat added between 40° and 160° is represented by the area $A_1 D_1 Z_1 Z_2 D_2 A_2$. The small squares on the diagram are each = one unit of heat. The part $A_1 D_1 D_2 A_2$ is the ergonal part, its specific heat rate is A D, which varies with temperature. The part $D_1 D_2 Z_2 Z_1$ is the thermal heat, that which constitutes the energy of the isochronous vibrations of the molecules of the liquid. The heat of the evaporation step at 160° C. is the area $D_2 Z_3$. The second law would say that in a perfect reversible engine working between 40° and 160° C. the working fluid would take in $D_2 Z_3$ of heat and perform D_2 *h* of work. The part $A_1 D_2$ has not been recognised before. I now explain that in such an engine the work represented by area A_2 *a d* D_2 is also done and this costs only the heat represented by the area $A_2 A_1 D_1 D_2$. When the range of temperature is insensibly small,

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a is sensibly on A_1 and *d* on D_1 and then the excess is as $A_2 D_2$ to $D_2 H_2$. The dotted curves V for constant volume and P for constant pressure are the loci of state-points, wholly in the gas field. Starting at any known pressure H_0 we can expand at constant pressure to P, and then compress at constant temperature to H_3, and the known thermodynamic relations enable us in this way to determine all the points in H, if we have a table of vapour-temperature pressures. Having the curve H we know that its tangent must cut the zero line Z as far to the right of its point of contact as is $= A_2H_2 - \frac{\gamma}{\mu(\gamma - 1)}$. This enables us to draw the initial boundary A_0 A, and so on. This was all explained in my Paper in 1880. This Paper is not intended to be a treatise on thermodynamics, or I would have to greatly extend these illustrations.

A cycle gas engine has its efficiency correctly expressed by the "second law," it would therefore be possible, theoretically, to combine the two to form a recuperating engine for winding up the universe again when its energy had run down too far. In the meantime I suggest that some attention should be paid to those millions of tons of hydrogen clouds which are so frequently belched out from the sun's photosphere, for I am inclined to suspect that I have been anticipated in these demonstrations of mine, and that they are but the exhaust blasts of such an engine which Some One has started long long ago. I, however, am of the opinion that such a forestalment would not be held to invalidate a patent in this country if any one should succeed in showing a way to making a living out of the excess above friction realizable by any combination on this plan. I wish inventors complete success on those lines, and I will not be jealous.

I have been requested to add a foot-note, explanatory of what may be to a few of the members of the Institution new expressions.

Thermodynamics.

The science of heat-forces and heat-power.

Thermodynamic Field.

The surface of a diagram representing proportionately, by area, the heat-energy of a substance, temperature being the vertical ordinate. The figure inserted is such a diagram.

Entropy.

Length upon a diagram whose area is heat-energy in thermal units, and the height temperature. That is to say, entropy is the horizontal dimension of the thermodynamic field.

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State-Point.

On the thermodynamic field the condition of a substance in relation to temperature, pressure, volume, energy, and molecular state is always fully defined by a single point or position, which is, therefore, called the state-point for that condition.

Ether-Pressure Theory.

The theory according to which the universal ether exerts enormous pressure upon all matter, and is in equilibrium with every body in nature, whether solids, liquids or gases.

Ergonal.

Belonging to work. In the ether-pressure theory the ergonal portion of the specific heat of liquids is the work which has been done upon the ether by the molecules in the extension of the amplitude of their inter-molar motions.

Vapour-Gas.

Gas at the pressure which the vapour would have at the same temperature.

Vapour-Gas Line.

The curve H. To the left of H is the vapour-field, to the right of it is the gas-field. The vapour-gas state-points are on the line H.

State-Points for Water.

The curve D, formerly it was the curve A.

The complete explanations will be given by me in a separate volume, which will shortly be in the press.

DISCUSSION.

Mr. F. W. Dick: My Lord and Gentlemen, I do not rise to address you at this time because I have anything to add to Mr. Gray's Paper, for to do that would take more time and more talent than I have at my disposal; but because from my training and my opportunities I think I have had a better chance of understanding Mr. Gray's investigations than most people. I heard about Mr. Gray's theory and work a good many years ago, and on each occasion on which I have visited London, I have taken care to increase my knowledge as much as possible, and I have come to this stage, that now I am quite prepared to disbelieve the second law of thermodynamics. I would point out to you that Mr. Gray has shown a remarkable series of agreements, so remarkable that they cannot be mere coincidences, and it has grieved and surprised me to see the small amount of attention that this subject has received. I do not know whether it

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is that business men are too busy to give such an abstruse subject the attention it requires or not, but I think it would be a great pity if we were to discover the true value of this investigation when it is too late. I would point out that, not only on the ether pressure theory has Mr. Gray shown this agreement, but also quite independently from the first law of thermodynamics. I hope that this subject will receive the attention it deserves at your hands, and will not be allowed to drop.

Professor J. H. Cotterill: My Lord, I will not attempt to discuss this Paper in detail: Mr. Gray has hardly, as yet, given us sufficient information to enable us to do justice to it; but I am happy to have the opportunity of making some general remarks. I admire the energy and determination with which Mr. Gray has prosecuted this subject for so many years, and I think the results he has obtained are so remarkable as to be entitled to a fair consideration by those interested in it. On publication of Mr. Gray's forthcoming work I shall endeavour to form a definite opinion, on the present occasion I can only touch briefly on a few special points.

First, we find the result of Mr. Gray's formula for the relation between the pressure and the temperature of steam agreeing very remarkably with the experimental results. Now that may be perfectly true, but it does not follow that the particular theory which suggested the formula is necessarily the truth. It may partially represent the truth, and no doubt does so, but it may not be the whole truth. I remember speaking on this subject once before, and giving as an example Rankine's formula, which was deduced from a certain theory as to the constitution of matter, which he described as that of Molecular Vortices. The formula closely represents the facts, and so do many other results deduced by Rankine from it, yet the author of it did not venture to say that it was true, nor is it in fact accepted as truth. I can now quote a stranger instance. In the *Comptes Rendus* for 1880, vol. 90, p. 1,079, M. Pictet obtains a formula much like Mr. Gray's; not, I think, identical, but involving the same quantities, and this formula agrees with extreme closeness, quite as closely as Mr. Gray's, with the experimental results, and like Mr. Gray's it is applicable to fluids other than water. I do not allude to this for the purpose of depreciating Mr. Gray's work. He has obtained a most accurate result independently. But I think M. Pictet's investigation shows that the perfect agreement of a formula with experimental results does not necessarily imply the truth of the ether-pressure theory.

Next, we find a column headed "Specific Heat, Second Law." Now I think that in calculating these results, Mr. Gray has assumed his theory and not merely the second law. Hence the discordance of the results with those found by experiment simply shows that his theory does not agree with the second law as he himself admits.

Mr. MacFarlane Gray: My theory has nothing to do with these results, and does not agree with them.

Professor Cotterill: I think I am right, as I will explain presently. Passing over some minor points on which I need not enter, I come to the basis of Mr. Gray's calculations. Let us consider that very interesting diagram which he has given us, as to which I may say that although Mr. Gray was not the first to conceive the idea of such a diagram, yet he was the first to apply it in detail. This diagram, though an interesting and eloquent method, does not in itself lead to results which could not

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be obtained in a different way. Now, I think, that on this diagram a hard and fast line is drawn between the vapour and the vapour-gas.

Mr. MacFarlane Gray: Yes.

Professor Cotterill: So I supposed, and here I entirely differ from Mr. Gray. Although that assumption may be, as a working hypothesis, very useful, and may lead to correct results in certain cases, yet I do not think it can possibly be true. We know that all gases are imperfect, that is, they do not exactly fulfil the laws governing the perfectly gaseous state. In the permanent gases the deviations are very small, as Sir W. Thompson has shown, but in carbonic acid they are five times as great, while in other cases they may be greater still. I think that in steam it must be the same. Now, whatever may be the ultimate fate of Mr. Gray's theory—as to which I will not speak positively—it appears to be inseparably connected with the assumption that the vapour passes into the perfect gaseous state beyond a certain sharply defined limit. Yet surely such an assumption is insufficient ground for a theory, which Mr. Gray tells us involves a perpetual motion furnishing us with any amount of power we please without cost. I know perfectly well, and have said so in my published work, that abstractedly there is no small difficulty in that reasoning about a reversible engine to which Mr. Gray alludes. It involves what appears to be, at first sight, an arbitrary restriction on the possible constitution of fluid bodies, but that reasoning has been accepted by all the greatest thinkers of our time, and I am quite sure that Mr. Gray will have great difficulty in upsetting it. I am convinced that he will have to resort to quite other arguments than those given in this paper. It is an accepted doctrine in science that energy tends to dissipation, and that its concentration by means of a perpetual motion is as impossible as its creation.

Mr. MacFarlane Gray: My Lord, I am pleased with what Professor Cotterill has said about my Paper, and I quite justify his remarks about not having all my work before him. I have read this Paper now to let the members know that I am still engaged on this investigation. I have not given all the mathematical details; I am still working at some of the corners of the diagram. I would like to make the step from ice to water as complete as I have already made that from liquid to steam, before I give it out of my hands. The complete investigation, mathematically, so far as I have gone, is on these sheets I hold in my hand. They have been handed about for inspection now. Mr. Dick, who has also spoken, has spent some hours over them to-day.

Professor Cotterill asks on what grounds I have made the line dividing vapour from gas a hard-and-fast line. I think I can clearly explain my reasons for doing so. Perfect gas is divided matter travelling in single molecules. Vapour is divided matter in which the travelling particles are not all single molecules. Vapour, then, consists of single molecules and mated molecules; perhaps a hundred single molecules and then a pair of mated molecules. My reasoning refers to single molecules only; for these in congregation there is a maximum pressure for each temperature. At that temperature-pressure or at that pressure-temperature the volume of a congregation of single molecules will be so much per pound weight of single molecules. The volume of a congregation of single and mated molecules will be at that temperature-pressure or at that pressure-temperature just the same, *so much* per pound of single molecules in it, added to the play spaces of the mated molecules. My reasoning has to do with the

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single molecule matter only, and the summation of the spaces occupied by the pound of single molecules will be the same at the same temperature-pressure, whatever be the arrangement of its volume elements, whether these form an unbroken contiguity of single molecule play spaces or are honeycombed for the play spaces of mated molecules, these mated molecules not being included in the weight of perfect gas, and the summation of the volumes of their play spaces being also left out of the measure of volume of the perfect gas. If the change of molecules from perfect gas into vapour were a gradual process this reasoning would not be tenable, but it is not gradual, the single molecules enter into the mated molecule state *per saltum*, there can be no intermediate state. The steam experimented on by Regnault was within a very small percentage of being wholly perfect gas; in, say, a pound-and-a-quarter of steam there was certainly always more than one pound of single molecules or perfect gas, and it is that pound alone I am dealing with at the critical pressure determining the change from singlehood to matehood at that temperature. That condition of temperature-pressure, with its necessary co-volume for single molecule congregation is a hard-and-fast condition, marking off, just as the hard-and-fast vapour-gas line $H H_0$ on my diagram does, the state of perfect gas from the state of vapour, the wholly-single-molecule state from the state in which the molecules are not all single. This critical pressure and co-volume for a given temperature applies not only to the envelope of a congregation of molecules but also to the play spaces of the individual molecules whose sum makes up the whole. It is, therefore, not material to this investigation whether true vapour-gas has ever existed by itself to the amount of one pound weight together or not, its mechanical properties, pressure and volume, and its thermal condition are the same, whether a few mated molecules be in the midst of their congregation or not, always leaving out of the reckoning the play spaces occupied by these mated molecules.

Professor Cotterill has mentioned that a Frenchman* in 1870 had done something on nearly the same lines. Of course, all formulæ of that kind must deal with latent heat, but whether he has dealt with latent heat in the way that I have dealt with it I do not know. Professor Cotterill was, when I read a paper on this subject at the Physical Society, in 1880, the only one who honoured me with criticism. At that time his only objection, if I remember rightly, was this, he said, "If you are right, then the gamma ratio must be the same for all substances, 1·4." I accepted this statement. He said it could not be the same for steam, because Regnault had by actual experiment ascertained that the specific heat of steam at constant pressure is ·48, whereas by my theory it would be only about 1·38. I said that I would look into the report of the experiments. Some months afterwards I replied by a Paper, "Regnault's Determination of the Specific Heat of Steam," in which I showed that Regnault had mis-read his own experiment and that the specific heat was, by these experiments, about ·38, as I had given it in my paper, instead of ·48, which it was supposed to be. The steam experiments upon contained moisture which was evaporated only after the temperature had been considerably increased. The specific heat by the experiments was ·38 from 212° up to 257° F., and during the completion of the evaporation at the higher temperature it was ·48. I think I have answered Professor Cotterill's objection to that.

* What Professor Cotterill alludes to, by M. R. Pictet, I find is published in the *Comptes Rendus* for 9th May, 1880. My printed Institution of Naval Architecture Paper was read on the 19th March, 1880. It is akin to what I am doing, but very different.—J. M. G.

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Professor COTTERILL: Is that Paper published? I have not seen it.

Mr. MACFARLANE GRAY: I wrote the Paper expressly in reply to your remarks: I took it to the Physical Society. It was published in the *Philosophical Magazine* and in the *Transactions of the Physical Society*, of which you were at the time a Member of Council; and you, Professor, have never even looked at it, although you have it on your bookshelves. I have a number of copies of it here, you can have one. I do not blame you. It is the same with everybody. Little attention is given to what other people do, every one is so much occupied with his own hobby.

The PRESIDENT: Gentlemen, it now behoves me, I am quite sure, on your behalf to thank our excellent friend here for his extraordinarily able Paper. Before the learned Professor rose I had a very strong disposition to think that our friend, Mr. MacFarlane Gray, was in the happy position of monarch of all he surveyed, and although his territory has been to some degree disputed, I think you will be quite content to leave him in quiet possession of his territory and all his nuggets till another time.

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ON THE APPLICATION OF MODERATE FORCED DRAUGHT TO THE FURNACES OF SMALL STEAM VESSELS, UPON MR. P. W. WILLANS' SYSTEM.

By Mark H. Robinson, Esq., Associate.

[Read at the Twenty-sixth Session of the Institution of Naval Architects, March 26th, 1885; the Right Hon. the Earl of Ravensworth, President, in the Chair.]

The application of forced draught to the furnaces of marine boilers, other than those of torpedo boats, has lately attracted so much attention, and has evoked so strong a desire for further information, that even a small and limited addition to our knowledge of the subject, if only it be based upon actual experience, may not be without use. As a member of the firm (Willans & Robinson, of Thames Ditton), by whom Mr. Willans' invention has been applied, I am able to describe a system of forced draught which has, at least, the advantage of having been tried practically, and successfully, and for a sufficiently long time, and in a sufficiently large number of instances, to enable a sound judgment to be formed upon its merits. But, lest too much be hoped from this preamble, I hasten to say that the field which the system covers is, so far, but a limited one, and that in respect of the vital question of economy, though a rough judgment can be formed upon it with some confidence, there are no quantitative results which can claim to have been arrived at with scientific accuracy.

Among the questions which had to be dealt with at Thames Ditton was that of providing satisfactory condensing machinery for small steam yachts and other small craft but more especially for yachts. Four years ago, when work there was commenced, the desire to use condensing engines in very small boats running in salt water was less general than it is now, when even the smallest steam cutter carried at the davits of a yacht is, or admittedly ought to be, fitted with some simple form of condenser. Nevertheless, the owners of the smaller classes of coast-going steam yachts, with non-condensing machinery, were alive to the advantages of condensing, and the question often arose—How to get the same power as before from an existing boiler deprived of its accustomed exhaust-draught in the funnel?

The size of yacht referred to is the small decked or partly decked steamer, whose engines indicate from 40 or 50 to 100 or 150 horse-power, and it is necessary to

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point out that this size of vessel lay under special disabilities. Smaller boats—such as open launches—had, as a rule, at that time non-condensing engines, and the sharp blast of the exhaust enabled them to work *with small and light boilers.* Larger vessels, on the other hand, had lofty funnels, and in that way gained draught and kept down the size and weight of their boilers. The small yacht (as just defined) was barred both by her size and by her owner's ideas of appearance and comfort from using either a high funnel, or that last resource of inefficiency, a strong and noisy steam jet. She was thus condemned, if her owner elected to condense, to work with a more sluggish draught than any other vessel, and, in consequence, to carry a boiler of such disproportionate size and weight as made it hopeless to look for high results in speed.

The same trouble, it may be remarked in passing, did not apply to the conversion of still smaller boats to the condensing system. It was not difficult to design vertical boilers for them with small water space and of light weight, which, though unsuited for working with either a strong draught or with salt water constantly renewed, were well adapted for natural draught and for use with a surface condenser, and which were able to give nearly as much steam per unit of weight as the hard driven boilers they replaced. Could similar designs have been accepted as suitable in larger boats the difficulty would have vanished, but such general acceptance has not yet been given, and the return-tube boiler still maintains its reputation for all but the smallest classes of machinery.

The question how to increase the evaporative performance of these low-funnel return-tube boilers, admitted, it must be owned, of an obvious answer—namely, that forced draught must be applied *to the moderate extent necessary to make up for the loss of the exhaust steam in the funnel.* This was clear enough, but the real question was how to do it?

The air-tight or closed-in stoke-hole was not, as a rule, admissible in a yacht, in whose internal economy it would introduce, or would be supposed to introduce, too many changes for the taste of the majority of owners. Besides, the question had some times to be considered with reference to open or partly open boats.

The use of a simple closed ash-pit, with air supplied under pressure, was still less desirable. With a greater pressure inside than outside the furnace door, no perfection of fitting would long prevent an escape of hot gases into the stoke-hole, a leak which could not fail to increase with age and use. Moreover, the injunction to first shut off the draught before opening the fire-door is one certain, sooner or later, to be forgotten, and then the

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opening of the fire-door is followed by an outrush of flame, to the serious personal danger of the stoker. Even a mechanical connection between the fire-door and the air-conduit, to close the latter when, or even before, the fire-door is opened, meets only half the difficulty at the best; it does nothing to prevent the constant leak of hot gases past the fire-door.

Desirable as it was, the satisfactory application of forced draught in such cases appeared to be nearly hopeless, until Mr. P. W. Willans, my partner, devised and patented the plan shown in Fig. 1. (Plate XIII.) A casing or chamber—a kind of mask—a few inches deep, is fixed to the boiler front over both the usual fire-door (hinged by preference at the bottom) and the opening into the ash-pit. The bottom of the casing is below the level of the stoke-hole floor, and is joined to a shoot which brings air from a fan. The front of the casing consists of a large door, which can be turned back or unshipped. When this is in place the only exit for the air, after it enters the chamber, is through the ash-pit, the fire-bars, and the funnel. The upper part of the casing is of course filled with air under pressure, and at a pressure slightly greater than that inside the furnace. Hence the leak past the fire-door is always in the right direction—*inwards.* The stoker cannot get at his fire-door for stoking without first removing the outer door, and the moment this is removed the boiler is working by natural draught, for the air from the shoot is as free to pass into the stoke-hole as into the ash-pit, and the pressure in the casing is destroyed without stopping the fan, or taking any other precaution. Danger to the stoker is at an end.

The casing or chamber is itself a small closed stoke-hole; so small that the man *stokes through it from the other side.* When forced draught is dispensed with the outer door is taken away, and the presence of the casing, or rather of its sides, offers no obstacle to stoking.

The first application of Mr. Willans' plan was in a small wooden coast-going yacht, the *Brenda*, of about 61 feet on the water-line, built by us in 1882. Displacement and draught were required to be small, for making trips to the Mediterranean through the French canals, and as high speed was also desired, it was necessary to restrict the size and weight of the boiler as much as possible. With this object Mr. Willans designed an oval return-tube boiler, 7 ft. long by 5 ft. wide by 6½ ft. high, stayed across. It had one furnace tube of 2 ft. 9 in. diameter, the fire-bars being about 4 ft. 3 in. long, giving a fire-grate area of nearly 11 square feet. The total heating surface was 375 square feet, which was, of course, very large for the size of boiler—a fact accounted for by the large proportion of heating surface in the tubes—330 square feet—in comparison with

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the whole. This was obtained by using a great number of small tubes—148, of 1¾-in. diameter. The tubes were of brass, and about 5 feet long. The form of boiler was convenient, as giving good bunker room at the sides, and it saved weight, in comparison with a cylindrical boiler, by somewhat restricting the water space. At the same time good provision was made for circulation. As might be expected, the large tube-surface extracted the heat from the gases most effectually, and the temperature in the uptake, though never tested with care, was low in comparison with that usual in small marine boilers. The working pressure was 120 lbs. The height of the funnel above the fire-bars was less than 11 feet.

With natural draught and careful firing, about 80-horse power was indicated. With a very light fan draught, not exceeding half-an-inch of water pressure, this power was approximately doubled. It was obvious that the boiler was in no way forced or overdriven, and no question of leaky tubes arose. It was, in fact, evident that at least 200-horse power might have been given by the boiler before the question of over-driving would have needed to be considered.

In comparing this boiler with others it must not be judged by its total heating surface, but rather by its capacity (which was roughly equal to that of a cylindrical boiler of 5 ft. 10 in. diameter)—or by its grate area, which was not purposely restricted, but was as large as was consistent with a good-sized combustion chamber beyond. On referring to the list of a well-known maker of small marine engines, I find that a cylindrical boiler of 6 feet diameter by 7 ft. 6 in. long is represented as having 300 square feet of total heating surface. This is with 58 tubes of 2½ in. diameter, and there is no reason to suppose that this boiler (which is larger than the *Brenda's* boiler) would not be capable of doing at least as much work, if its tubes were as small and as numerous, and if the draught were forced to the same degree. Tubed as described, and worked with natural draught (probably with a higher funnel), it is specified by the maker as suitable for a compound condensing engine of no more than 90 I.H.P. With little or no alteration, beyond an increase of tube surface, and the application of moderate forced draught, it seems probable, therefore, that its evaporative power might be *more than doubled*, and—as will appear presently—without loss of economy.

The engine of the *Brenda* was an ordinary Willans' Patent Compound Surface Condensing Engine, with low-pressure cylinders of 12¼ in. diameter and 10 in. stroke, able to indicate 150 H.P. at 360 revolutions and 120 lbs. steam pressure. As economy in this vessel was deemed less important than light weight, a low ratio of expansion was adopted—under five to one,

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and the coal consumption, as ascertained by numerous trials, all somewhat rough, but reliable in the aggregate, lay between $2\frac{1}{2}$ and $2\frac{3}{4}$ lbs. per I.H.P. per hour. This was when working at moderate powers—up to about 120 H.P. No reliable tests were made at higher powers, but the consumption did not appear to increase in any marked proportion. Nor was it likely to do so, because at the highest powers the sharpness of the draught was far below that used in many non-condensing vessels, such as the ordinary high-pressure Thames tugs, for instance. In the *Brenda* it need not be said that the forced draught was inaudible, and was in no way a nuisance on board.

The fan draught thus amply fulfilled its purpose of combining power with lightness, and the yacht achieved a high-rate of speed as intended.

With regard to other matters it was found, first, that while the comfort of the engine-room was increased, as might be expected, by the ventilating effects of the fan, the labour of stoking, *and particularly the skill required in stoking*, were very much diminished.

In small return-tube boilers, working with a slack draught, it is necessary to keep a thin fire, and it need not be said that frequent attention and some skill are required to prevent a thin fire from burning through into holes. When the yacht was doing her best with natural draught (making rather over 10 knots), the fire-door required to be opened about once in twenty minutes. When doing the same speed (which chanced to be the ordinary working speed) under fan draught, the fire-door was opened in practice *only once an hour*. On one occasion the yacht ran from Teddington Lock to beyond North Woolwich—*over* 29 *miles*—without opening the fire-door, and was by no means without a fire at the end of the run. On another occasion she ran from Greenhithe to Westminster (22 miles) partly against tide, without opening the fire-door, and arrived, in exactly two hours, with a fairly good fire. The explanation lay in the fact that, with the strong draught, a very thick fire could be carried, thick enough neither to need replenishing for a long time, nor to be in danger of burning through into holes. However, for practical purposes, it was sufficient that stoking was brought to be but an hourly event, and that the chief difficulties of stoking a small return-tube boiler were removed. The system, as may be supposed, has always been liked by the engineers of the boats, whose reports have been uniformly favourable in the six yachts in which, up to present time, it has been applied and practically tested.

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It is hardly necessary to say that with a locomotive boiler much longer runs might be made than those just recorded, because in a fire-box of locomotive type a very much deeper fire may be used than is possible in the contracted furnace flue of a return-tube boiler. It is possible that this fact may not be without influence upon the construction of marine boilers in future, when the advantage of a deeper fire than the ordinary return-tube boiler affords has been recognised. But in hazarding this opinion I have no intention of recommending the adoption, for general work at sea, of what is now known as the locomotive type boiler, the practical objections to which are thoroughly understood.

Secondly, at powers within the range of natural draught, and at which alone comparison was possible, it appeared that not only was there no loss of economy, but that a distinct though small gain resulted from using fan draught. The gain was sufficient to be noted in practice, as the result of repeated runs, but it was not established by direct experiment. It has, however, been confirmed by all our later experience with moderate forced draught. Its cause was no doubt the greater advantage to which the coal was burnt in a thick than in a thin fire; reduced chances of mischief from unskilful stoking; and the less frequent disturbance of the fuel in putting on fresh coal.

In the first apparatus fitted, it was found that when the two doors were open, and the fan running, the air from the shoot passed too close to the furnace door, and was apt to draw out from the furnace, and carry into the engine room, the light particles of ash lying near the opening. Mr. Willans therefore designed the form shown in Figs. 2, 3 and 4 (Plate XIII.) in which the outer door A, which turns upon a spindle B at the bottom, and lies upon the floor of the stoke-hole when opened, has a prolongation C, which is raised against the top of the air-shoot while A is shut, but descends and closes the air-shoot, as A is opened. The spindle B carries a crank or lever, connected by a slotted link with another lever on the spindle D, which carries the fire-door F, in such a manner that F does not commence to open until C has closed the air-shoot (though with an easy fit), and has practically shut off the blast. This plan, which appears to work quite satisfactorily, has been applied in several cases, one being that of a steam fishing yacht of about 40 tons, the machinery of which, of 100 I.H.P. is shown in Fig. 4. (It should be remarked that the depth of the air-duct or shoot, under the floor plates, might have been considerably less than is shown in the figure. Had saving of space been an object, the necessary area could have been obtained by making the shoot wider and shallower.)

Another almost duplicate set of machinery has lately been sent to America, in the

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hope that the fan draught will enable anthracite coal to be burned in a boiler otherwise of English type, and a third similar set is ready for shipment to the same destination.

For the sake of simplicity the fan has hitherto been mounted upon, and driven by the main engine. There are many advantages in using an auxiliary engine, but in small yachts one of the elements most to be considered is the possible inexperience, or even neglect, of the engineer, and driving by the main engine not only gives him an engine less to attend to, but makes it impossible for him to run the fan and raise steam improperly, while the engine is standing or is working at low power. It also ensures, in a rough way, through the varying speed of the engine, that the greater the power used, the more air shall be supplied. In Fig. 4 it will be seen that the fan is mounted in a cast-iron casing, which is combined with the forward main bearing of the engine. The shaft passes through the casing, and by a crank on its forward end, works the air-pump and feed pumps. The air-pump is inverted, and is attached to the fan casing. In some examples only the lower part of the casing is of cast iron, the upper part being of thin iron or steel, for lightness. The fan casing does not stand vertically at the end of the engine, but leans over to the port side, so that the spindle of the fan may be continued aft, past the back of the engine. At G is a pulley, by which it is driven from the larger pulley H, formed upon the main crank shaft coupling. Several methods of driving have been practised. Steel bands, running upon leather-faced pulleys, and with tightening pulleys for taking up the slack, have been used; so have pitch chains, and Gandy's cotton belting, sewn endless, has also worked well. That no kind of belt driving is always perfectly satisfactory on ship-board, is unfortunately true, but the small liability to temporary failure which exists, is the price paid for getting rid of a separate steam engine. Even if a failure happens it cannot be said that in a yacht any very vital interest is jeopardised, while repair, if necessary, is not more than a question of minutes. In a vessel of larger size it would of course be better to use a separate engine.

It is worth noting that the ventilation given by the fan is so much valued that the owner of the last-mentioned yacht has had a door cut in the lower part of the fan casing, so that the fan may deliver air into the engine room when it is not required for the furnace. In this case it must be rather circulation of air, than the introduction of fresh air, which is effected, but its action seems to be found beneficial.

The removal of ashes from the ash-pit is easily effected by shipping a temporary bridge across the bottom of the chamber. Any ashes, &c., which may fall into the shoot are

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removed by taking up a well-fitted floor plate in the stoke-hole, which forms a portion of the top of the shoot or conduit from the fan to the chamber. Absolute tightness in the shoot is unimportant, with such low air pressure.

It is unfortunate that, as stated in the beginning, there are no really accurate figures to be placed before the Meeting.

It may be assumed that directly a steam yacht is ready, the owner wishes to take her away, and the opportunities given to the builders for carrying out anything like continuous and systematic trials, are very few indeed.

But from the approximate figures and the general facts given, it may be said with confidence that Mr. Willans' plan both meets an obvious want, and meets it successfully. The class of vessels to which it most directly applies, though numerous, is not, of course, an important one, from the point of view of the shipowner and shipbuilder, and its chief interest for them will lie in the inquiries, first, whether the system has any probable application in larger vessels, and, secondly, whether from the limited experience with it described in this Paper, there result any suggestions towards the extended application of forced draught generally.

To the first inquiry, as a builder of small vessels only, I can offer no answer. That must come from the shipbuilder, the marine engineer, and the owner, who will know best whether the efficiency and comfort of the stoke-hole will be better promoted by enclosing it under more or less air-tight hatches, or by leaving it open and providing the furnaces with double doors upon Mr. Willans' plan.

The general suggestions to which our limited experience give rise are, I think, these :—

First, that systematic experiments should be made to ascertain the strongest draught under which coal can be burnt with advantage, *varying the construction of the boiler with the increasing draught.*

Second, that for the purpose of such experiments, and generally, it may be laid down that the variations necessary are three in number :

(*a*) Provision should be made for working with thicker fires, as the draught is increased.

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The well-known facts of locomotive practice are alone sufficient to suggest the propriety of this.

(*b*) As the draught increases, so should the size of the combustion chamber, in order that combustion may be completed before the gases enter the tubes, the smallness of which is unfavourable to the continuance of combustion.

(*c*) With stronger draught there should be larger heating surface, obtained chiefly by reducing the size of the tubes and increasing their number.

The two last suggestions are also obvious enough, because both the space for completing the process of combustion, and the surface provided to absorb the heat produced, should clearly vary, rather with the quantity of hot gases, than with the size of the boiler or the area of the fire grate.

As to the tubes, our progress has been steadily in the direction of smaller tubes. We are now using $1\frac{1}{2}$-in. brass tubes instead of $1\frac{3}{4}$-in. for lengths of about 4 feet, and should not hesitate to use them for greater lengths. Small yachts, no doubt, are expected to burn smokeless coal as a rule, but we have had some experience of $1\frac{3}{4}$-in. tubes with very smoky Scotch coal, and the small tubes in that case caused no inconvenience, though the boiler worked only by natural draught. The sharper the draught, the less, of course, is the tendency to foul.

In conclusion, it should be once more pointed out that the success achieved in the vessels fitted upon Mr. Willans' plans must not be looked for, as of course, in other and different vessels. Larger vessels begin where we, so far, have left off. Our object has been to raise artificially the rate of combustion in small vessels, to that hitherto obtained naturally in large vessels; to raise still further the rate of combustion in large vessels is a different matter. But I venture to hope that it may eventually be treated successfully upon the lines just indicated.

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DISCUSSION.

Mr. M. H. Robinson: My Lord, before replying to the questions put to me, I should like to be allowed to say, that after this Paper was in print, in fact, only last evening, Mr. Thornycroft was kind enough to explain to me certain arrangements, securing some of the ends which Mr. Willans had in view, which he applied in one of his launches some years before Mr. Willans took up the subject. The plans are so far different, that I do not know that any further reference to the matter is required; but the fact that Mr. Thornycroft had thus moved in it so long ago, ought to be recorded in connection with this Paper. I may add, that the fact was as new to Mr. Willans as it was to myself.

Mr. R. J. Quelch: I think in common fairness to the author of this Paper, Mr. Robinson, I ought to make some remarks on the Paper that we have had. I must say that I came into the room late, and therefore I did not see Mr. Robinson, and it is quite two years ago since I had the pleasure of meeting him, being appointed by the owner to test that steamer the *Brenda*, before she was accepted, and I must say this, that, if I had not been prevented, having to leave very hurriedly after testing this steamer, to go to Egypt, I personally would have had some experiments made to test the economy of the thing out of curiosity myself, the owners not caring about it. I was interested in it, and I think Mr. Robinson also, but I was prevented, and I think he was also. I have not had any communication with Mr. Robinson since then. I have not seen him since we had that trial, and therefore my testimony, I suppose, would be perfectly unbiassed. I thought when the steamer was first contracted for it was not intended to use the forced draught. That was put on afterwards to get the increased speed, the owner was very doubtful himself about the effect of it on the boiler, and I was instructed to examine the boiler after the speed trial, and after a very searching examination I must say that I never saw a lighter boiler than that boiler was. I examined her very carefully, and also in the runs we had down the river on the measured mile. I was astonished at the burning of the fires, and the little stoking that was required. It was a perfect success. There was one thing that was not a success, and which, perhaps, is not germane to the principle which we are discussing here, which was, that Mr. Robinson, like a great many other people, unfortunately undertook to produce speeds in certain sizes of boats, which it was perfectly impossible to do, and he has got into trouble in that way, but that does not touch this question. We could not get the speed we wanted, although the boiler and draught were perfect successes. In the Paper here, I think, there is a discrepancy in what Mr. Robinson tells us. He says, that "As the draught increases so should the size of the combustion chambers, in order that combustion may be completed before the gases enter the tubes, the smallness of which is unfavourable to the continuance of the combustion." I do not know what the general experience of engineers is on that point, but I must say that my own opinion is that the heating surface of the tubes is generally far too great in the great majority of marine boilers, and in every case where I have replaced an old boiler with a new one, I have enlarged the combustion chambers, in the ordinary cargo boats I have to do with as superintendent engineer. I have increased the size of the boiler a little as far as steam room is concerned, and taken out the rows of tubes, that I am afraid shipowners are rather fond of putting into boilers. I have known a case in

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which a shipowner has just taken the specification, and added so much to the heating surface without doing anything more, and things of that sort, and the result is, we get boilers filled up; perhaps it is not the fault of my brother engineers, putting overheating surfaces, when such things are done in specifications, and therefore I do not blame them for that; but I may say in taking off the tubes frequently, a row at the top and a row at the bottom, and pitching the tubes further apart which is an idea of my own—by taking out the tubes and enlarging the combustion chambers, I have got far better results than in ships in ordinary practice. Mr. Robinson goes on to tell us after that, "as to the tubes our progress has been steady in the direction of smaller tubes," after telling us that a small tube is unfavourable to the continuance of combustion. Therefore I think if Mr. Robinson would put in fewer tubes and enlarge his combustion chambers still more in proportion, he would find he would get a far better result.

Mr. H. H. West: My Lord, I have listened to this Paper with a good deal of interest. I should like to ask Mr. Robinson a question in reference to the amount of pressure of blast he had, for I have only found one reference to it, and if I rightly follow the paper it is half an inch of water. I would like to know whether that pressure of blast applies to all the results he gives in the paper. In a little steam launch that I am concerned with at present, we had this difficulty; the vessel was intended to go to a place where fresh water was dearer than coal. That is, coal had to be used to make it, for *all* fresh water there is distilled from the sea. It thus became practically an impossibility to use high pressure steam with an ordinary non-condensing engine, and we were driven to use a compound engine. Now I was a little afraid of a compound engine. My fear was, that we should not be able without a blast in the funnel to get sufficient draught to maintain the combustion which we required, and I was a good deal concerned lest there should be a failure in consequence of this; however I am happy to say that the other day I saw a little vessel built by Messrs. Cochrane and Company, of Birkenhead, in which they had an ordinary natural draught boiler and compound engines, a little larger than those that Mr. Robinson refers to here in his paper. I think they were 8 and 16 inch cylinders, and the steam was maintained perfectly at the working pressure with a natural draught. The only peculiarity of the vessel was, that perhaps some people would say she was unsightly on account of the length of her funnel. For my own part I think the measure of beauty is utility, and if the length of the funnel increases its utility I do not think anybody should complain of its appearance. I mention this because at the commencement of the Paper Mr. Robinson seems rather to doubt the propriety of using this means of creating the draught.

Mr. Mark H. Robinson: My Lord, it does not appear that there is very much for me to answer here. Mr. Quelch has alluded to some matters connected with the yacht first referred to, which I think are not germane to the present question. The simplest answer to his remark about her speed is the fact that she was built under penalties, and that she was paid for in full. Mr. Quelch is not, I think, aware of all the circumstances connected with this matter, and to the best of my recollection he was only on board upon one occasion of the vessel being under way. His assumption that the application of forced draught was an afterthought is entirely incorrect. The yacht was designed with it from the commencement. Then as to combustion of the gases before they enter the tubes,

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that really is a matter with which I ought not to deal, because it requires much greater chemical knowledge of the question of combustion than I possess. But I know it is the view of very high authorities, and it seems to me a reasonable one, considering that we want the gases to be cool when they appear in the uptake. They will hardly be so cool as we wish to find them within a few inches only of the place where they have been in a state of actual combustion. We want combustion to be complete before they enter the tubes, partly for this reason and partly, because if the tubes are small enough to really break them up efficiently so as to get the heat out of them before they reach the uptake, there is danger that any unfinished combustion will remain unfinished on account of the low temperature in the tubes; if so, instead of carbonic acid we shall pass carbonic oxide into the uptake, where (or at the top of the funnel) the process of combustion is only too likely to recommence. Depend upon it, the best place to finish combustion in is the combustion chamber. I quite agree with Mr. Quelch in his remark that a decrease in the number of tubes is often a good thing. Frequently in a return-tube boiler half the lower tubes get choked up without the smallest difference in the steaming. That is a common experience, and I quite agree with Mr. Quelch about it, but I think it has no bearing upon the use of small and numerous tubes with forced draught. In the one case you have a weak natural draught inviting the gases, so to say, to come through the tubes, in the other, if I may so express myself, you get behind them and shove them through, and I think that alters the case a good deal. With regard to Mr. West's remarks, which touched chiefly upon the height of the funnel, I agree with him entirely that it is a simpler and more business-like job to heighten the funnel if you can heighten it sufficiently. But in our small work we have to deal with private owners, and in the case of the small vessel referred to, the low funnel (I think only 10 feet above the bars, instead of eleven, as stated) was forced upon us as an absolute condition. It is quite a common case with the owners of small steam yachts, and it must be remembered that not a small addition, but a very great addition to the height of the funnel would be needed to give an effect at all corresponding with what we obtain with the fan. The water pressure, no doubt, was guessed at somewhat roughly, we tried it only with a common gasfitter's gauge. Mr. Willans, who conducted such experiments as we were able to make, had had considerable experience before in forced draught at higher pressures, and I know his opinion was that the pressure was a very light one, never more than half an inch on those occasions. Further than that I do not think I have anything to answer.

The President: You will allow me to thank Mr. Robinson for his very interesting Paper.

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ON THE STRENGTH OF MILD STEEL PLATES AND RIVETS OF VARIOUS KINDS USED IN SHIPBUILDING.

By J. G. Wildish, Esq., Member.

[Read at the Twenty-sixth Session of the Institution of Naval Architects, March 27th; the Right Hon. the Earl of Ravensworth, President, in the Chair.]

The importance attaching to the strength of the plates and rivet fastenings in ships has been generally recognised from the time that iron took the place of wood in their construction. In a Paper contributed by Mr. Henry H. West to the *Transactions* of last year, it was shown that the disposition, according to the usual rules, of the rivets in the skin and other important parts of the hull structure of iron-built ships, requires modification to secure a proper balance between the strength of the plates and the rivets; and that much useful information on this subject still remains to be furnished. In that Paper, the author contends that the deficiency—which he instances in detail—in the sectional area of the rivets, as compared with the reduced area of the plate section at the line of the rivets, is a serious imperfection, and he arrives at the conclusion that the present practice of iron shipbuilders does not secure the most efficient and economical joint.

Much consideration has already been given to the strength of the plates and rivets used in ship construction, and in venturing to bring this question under your notice again, it may be desirable to state at once that I have been led to do so from the fact that the Director of Naval Construction has kindly given me permission to communicate the results of some recent experiments made at Pembroke Dockyard, under the superintendence of Mr. J. C. Froyne, the Chief Constructor there, and his staff, in order to confirm or correct the data previously obtained as to the tensile strength of mild steel plates after punching, drilling, or countersinking; and as to the shearing stress of the different kinds of rivets in ordinary use in the service.

From the experiments made at Chatham about twenty years ago for guidance in determining the strength of the riveted joints in the ships built of iron in the Royal Navy, the shearing stress of a $\frac{3}{4}$-inch iron rivet of Lowmoor or Bowling quality was ascertained

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to be 10 tons, when connecting two iron plates together; the shearing stress of other sized rivets being found to vary in proportion to their sectional area. The double shear of the $\frac{3}{4}$-inch rivet was shown to be about 18 tons. These results are as given by Sir E. J. Reed in his work on "Shipbuilding in Iron and Steel," and the tensile strength of the unpunched first-class iron plates was taken at 22 tons per square inch, by the same author, in his exemplar calculations for the strength of riveted joints; and at 18 tons per square inch for the strength of the plate sections at the line of the closely spaced rivets in the butts.

In the Navy the earlier ships built of steel had their plates connected together with iron rivets, as suitable steel rivets had not then been manufactured. But it soon became evident that iron rivets in steel plates was not a desirable combination. The tensile strength of the steel plates exceeded that of the first-class iron plates, which had formerly been used in the important parts of the structure, such as the skin, upper deck, &c., by something like 30 per cent., while the shearing stress of the iron rivets when used in the steel plates was found to be considerably less than when used for connecting the iron plates together.

Before commencing the outside plating of the *Iris* and *Mercury*, the first of our steel-built ships, some experiments were made which led to the conclusion that the single shearing stress of the $\frac{3}{4}$-inch iron rivet in the $\frac{1}{2}$-inch plates intended to be used would be only 9 tons; and that to provide the requisite strength in this plating, it would be necessary to reduce the pitch of the rivets in the butts to $3\frac{3}{8}$rds diameters. The plates in these ships were annealed after the holes were punched, which with the countersinking wholly or at least largely removed the injurious effect of the punch; and in estimating the longitudinal strength of the plating, 27 tons per square inch was allowed for the strength of the plate sections at the line of the rivets in the butts, 29 tons per square inch being taken for the strength of the unpunched parts of the plates.

Some further experiments were made at Pembroke in 1878 with iron rivets in steel plates, the details of which are given in Appendix I. $\frac{9}{16}$-in. and $\frac{3}{4}$-in. plates were used, made by the Landore Steel Company, the rivets for connecting the test pieces, which were jointed with a double riveted strap to represent the butts of outside plating, being $\frac{3}{4}$-in. and $\frac{7}{8}$-in. respectively. In some of the tests the countersinking of the holes was carried right through the plates, and in others to within $\frac{1}{16}$th of an inch of the full thickness. This variation, however, gave no appreciable advantage either way; but from these experiments it appeared that the average single shearing stress of the $\frac{3}{4}$-in. iron rivet in steel plates was only

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8·1 tons as compared with the 10 tons for the same rivet in iron plates. The mean single shearing stress of the $\frac{7}{8}$-in. rivet was $11\frac{1}{2}$ tons, which, after allowing for the difference in size, is a somewhat better result than just given for the $\frac{3}{4}$-in. rivet, but is still 2·1 tons less than for the same rivet in iron plates.

This comparative weakness of the iron rivets when used for connecting steel plates was met by making the rivets larger, as well as by placing them closer together; but the larger rivets involved broader laps for the plating, thus objectionably increasing its weight as a whole.

Precisely similar experiments to those just alluded to were made in 1880, except that steel rivets were used instead of iron. The results of these experiments (which are also given in Appendix I.) were exceedingly uniform and satisfactory. They showed that $11\frac{1}{2}$ to $11\frac{3}{4}$ tons might be allowed for the single shear of a $\frac{3}{4}$-in. steel rivet in steel plates, and $14\frac{3}{4}$ tons for a $\frac{7}{8}$-in. rivet.

Great care was taken in the manufacture of the steel rivets; and to ensure their being of uniformly good quality, a code of tests was prepared for guidance in making them. A copy of this code of tests, which is still in use, is added at the end of this paper, the detailed dimensions of the rivets being also given.

With regard to the treatment of the steel plates in working them into the hull, it may be well to remark that as long ago as 1878 it was decided not to anneal them after punching, as a means of making good the injury to the material due to the punching. All the butt straps, however, to the plating, forming an important feature in the general structural strength, such as the outside plating, deck plating, stringers, &c., were ordered to have the holes drilled in them, or to be annealed after the holes were punched. The countersunk holes in the outside plating were at the same time ordered to be punched about $\frac{1}{8}$th of an inch smaller in diameter than the full size required, the enlargement of the holes being made in countersinking, which was to be carried through the whole thickness. Countersunk rivetting was also to be adopted in the stringers, deck plating, and other parts subjected to considerable tensile strain, the holes being treated similarly to those in the outside plating.

This practice is now followed in the service, and the annealing, both of the frame bars as well as the plates, is only carried out in very exceptional instances, and in order to relieve the tensions set up in the material in bending it to shape.

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We now come to the experiments, made at Pembroke in 1883 and the following year, to which I would desire to direct your special attention. The detailed results of these experiments, which were of a more comprehensive character than those made previously, are recorded in eight tabulated statements which are added to this Paper (see Appendix II.) The first set of experiments made, comprising sixteen separate tests, were intended to illustrate the effect on the strength of the steel plates, of punching, drilling, or punching and then countersinking holes in them, in a cross section. Twelve of the test pieces, half being cut from one $\frac{1}{2}$-inch plate and the remainder from another $\frac{1}{2}$-inch plate, were shaped according to the sketch B (see Table 1). The other four pieces were cut in the form of parallel strips, to be used for obtaining the tensile strength of the plates before perforation. The results with these strips gave a mean breaking stress of $28\frac{1}{4}$ tons per square inch of sectional area. Of the pieces shaped to the sketch B, two cut from each large plate had two $\frac{7}{8}$-inch holes punched in them, in a cross section; two others from each plate had $\frac{7}{8}$-inch holes drilled in the same positions, the pieces from one plate having the holes countersunk as well; and the remaining four pieces had the holes punched $\frac{3}{4}$-inch and then increased in countersinking to $\frac{7}{8}$-inch minimum diameter. The gross breadth of the section across the holes was about $5\frac{1}{4}$ inches in each case, the pitch of the holes being three diameters. In the tests with the punched holes in the cross section, the mean tensile breaking stress per square inch of the remaining material in the section at the holes was but just over 22 tons. In the cases with the drilled holes the corresponding result was $29\frac{1}{2}$ tons nearly; and in those with the holes punched small and countersunk to the full size and usual taper, the mean breaking stress per square inch was just under 29 tons. There was but little variation in the individual results with the same kind of hole. With reference to the test pieces having the holes punched, including those afterwards countersunk, it should be remarked that they were shaped *after* the holes were punched.

The most striking fact in connection with these results is the considerable degradation that appears in the breaking strength of the remaining material in the sections through the holes punched the full size, which is shown to have been reduced about 22 per cent. by the punching. The elongation before rupture was also limited, being only ·1 to ·2 of an inch, while the elongation with the drilled holes was from ·29 to ·4 of an inch. This reduction in the tensile strength of the remaining material across the holes, caused by the punching, was not unexpected, the injurious effect of the punch being already well known. The exact percentage of reduction shown must not be regarded as applying generally to plates of other thicknesses, and having the holes about the same relative size and distance apart. Moreover the size of the punched holes as compared with the thickness

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of the plates, and also the size, in relation to that of the punch, of the bolster or die used in making the holes, no doubt influence the strength of the remaining material in the section across the holes.

It may not be out of place here to refer briefly to the excellent experiments made by Mr. Kirkaldy, 3 or 4 years ago, with mild steel plates from the Steel Company of Scotland, for the information of the Board of Trade. Plates 1-inch, $\frac{3}{4}$-inch, $\frac{1}{2}$-inch and $\frac{1}{4}$-inch thick were used. The 1-inch plates had four holes 1 inch in diameter, the $\frac{3}{4}$-inch plates had four holes 1 inch in diameter or six holes $\frac{3}{4}$ inch in diameter, and the $\frac{1}{2}$-inch and $\frac{1}{4}$-inch plates had six holes $\frac{3}{4}$ inch in diameter, punched in a cross section 12 inches broad in each case. In the experiments with the 1-inch plates the tensile strength of the net section at the holes was reduced 24 per cent. by the punching; with the $\frac{3}{4}$-inch plates the reduction was $17\frac{1}{2}$ per cent., the variation in the size of rivet giving no advantage; with the $\frac{1}{2}$-inch plates the reduction was only 6 per cent., and with the $\frac{1}{4}$-inch plates it was *nil*.

Returning to the experiments now before us we proceed to observe that with the drilled holes the breaking strength of the remaining material in the section was somewhat greater than for the solid or unperforated part of the plates. This should not I think be regarded as accidental. The experiments of a similar nature, made by Mr. Kirkaldy, showed a marked increase in the stress per square inch of the net area between the holes, as compared with the stress for the solid part of the plates. This has been attributed to the limitation in the length over which the elongation takes place. With the holes punched $\frac{3}{4}$-inch and countersunk to $\frac{7}{8}$-inch diameter (at the smaller end the taper being as usual) the injurious effect of the punching was entirely removed.

The next set of tests we have to consider has reference to the strength of the steel plates when riveted together by means of a single strap like the butts in the outside plating of a ship. Fourteen tests of this kind were made, the plates, which were nominally $\frac{1}{2}$-inch thick, being shaped and connected together as shown on sketches C and D (see Table 2). In eight cases the strap was double riveted with four rivets in each plate, or eight in the strap; and in the remaining six, the strap was treble riveted, with six rivets in each plate. Four of the double riveted straps were fastened with $\frac{3}{4}$-inch rivets, the breadth of the plates at the reduced part being about $4\frac{1}{2}$ inches. The rivets in two of these straps were pan headed and countersunk pointed; and in the other two, pan headed and snap pointed, the points in each case being knocked down on the plates. The fastenings in the remaining four double riveted straps were similarly arranged, but the rivets were $\frac{7}{8}$-inch, the breadth of the plates at the reduced part being about $5\frac{1}{4}$ inches. The spacing of the rivets corresponded with

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a pitch of three diameters, the breadth of plate at the reduced part being equal to six diameters or two pitches. The holes in the plates for the countersunk pointed rivets were punched smaller than required, as usual, and enlarged in countersinking to the full size for the rivet. The holes for the snap pointed rivets were all punched to the full size.

In the plates with the punched and countersunk holes, the mean breaking stress at the section across the holes amounted to 28·9 tons per square inch of net area, showing that the material had not been injured by the method adopted in making the holes. In the plates with the holes punched the full size required for the snap rivets, the mean breaking stress per square inch at the weakened section was 24·9 tons.

In the six other tests completing the set, the treble riveted straps were secured with $\frac{3}{4}$-inch pan headed and snap pointed rivets; the holes in the plates being punched the full size as before. These tests were arranged in three pairs to suit a varying pitch of rivet, in steps of $3\frac{1}{2}$, 4 and $4\frac{1}{2}$ diameters, the breadth of the plates at the reduced part being 5·1, 5·45 and 5·95 inches respectively. In one instance the breaking stress of the plate at the section across the holes was exceptionally high, amounting to $27\frac{1}{4}$ tons per square inch of net area, but in the other five, in which the results did not differ much, the mean stress per square inch was a little over $24\frac{1}{2}$ tons.

This stress, or the 24·9 tons just mentioned for the double riveted straps, when compared with the 22 tons per square inch previously given for the punched but unriveted plates, and with the stress for the solid plates, would appear to show that when $\frac{1}{2}$-inch plates are connected together with $\frac{3}{4}$ or $\frac{7}{8}$-inch rivets in the manner described, their ultimate tensile strength at the section across the punched holes is increased by the riveting, and is only 12 to 13 per cent. less per unit of net area than for the solid part of the plates. This recuperative effect apparently of the riveting on the strength of the plates at the section across the punched holes, is difficult to explain. Being connected by a single strap, the test-pieces would bend somewhat at the holes before finally separating; and the section there would be subjected to a tearing stress. This latter might have been expected to still further degrade the ultimate strength of the section, as in the case of iron plates. It may, however, be that with such ductile material the bending referred to had the effect of reducing the injury done by the punch, and that this reduction was not due to the mere process of riveting. This view is supported by two cases to be afterwards described, in which the plates, which were connected with double straps, broke across the punched holes, at a stress per square inch of about 23 tons. All the test-pieces in this set were cut from the same plate in order to obtain the most reliable results.

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In estimating the longitudinal strength of the plating in ships of recent construction in the Navy it has been usual to allow 26 tons per square inch for the breaking stress of $\frac{3}{8}$-inch to $\frac{5}{8}$-inch steel plates at the sections through the holes in the butts, and in line of the closely-spaced rivets at the watertight frames, the holes being punched small and countersunk to the full size. From the experiments now before us and the set previously considered, it would appear that as much as 27 to 28 tons per square inch might be taken for the tensile strength of the plates at the sections referred to. The results of these experiments are confirmatory of the practice followed in the Service, as to the treatment of the plates in working them into the hull. In cases where double butt straps are used the rivets being closely spaced, and the maximum strength is required in the connection, it is seen that considerable advantage is derived from drilling the holes in plates, instead of punching them. It may be well to add, that the tests with the strapped joints having the same spacing of rivets showed the strength of the plates across the holes, per unit of gross sectional area, to be nearly the same, whether the holes were punched small and countersunk to the full size, or punched the full size required for the snap riveting. The countersinking removed the injurious effect of the punch, but it, of course, reduced the sectional area of the plates as well, and the one about balanced the other. This suggests the question whether the precaution of drilling the holes in the straps or annealing the straps after punching might not be dispensed with, in some cases; though, on the other hand, the annealing is effected with but little trouble or expense, and puts the strap beyond doubt.

The next three sets of experiments (see Tables 3, 4 and 5) were made to determine the single shearing stress of different sizes and kinds of iron screw, and steel, rivets when used in steel plates. Sixty-six separate tests of this kind were made, the nominal diameter of the rivets being $\frac{3}{4}$ inch, $\frac{7}{8}$ inch and 1 inch. The two smaller sizes were used in $\frac{1}{2}$-inch plates and the 1-inch rivets in $\frac{3}{4}$-inch plates. The plates were cut to the general shape shown on sketch C, and were connected together by single straps of the same thickness as the plates, the holes in the straps being drilled. All the holes for the hammered rivets were made $\frac{1}{16}$th of an inch larger than the nominal diameter of the rivets, to allow the usual clearance. With four exceptions, the straps were double-riveted, four rivets being used in each plate. In the exceptions referred to the straps were treble-riveted, with six rivets in each plate.

The tests with the iron rivets were made to ascertain the shearing stress of the different kinds of screw rivets in common use. The continuous screw rivet, tapped into both plate and strap, and cut off flush—like the fastening adopted in certain parts of the upper structure of war-ships to avoid the danger of the rivet-heads flying off in action—was first

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taken; then the hexagonal headed screw, tapped into the strap only; and, lastly, the screw pointed and countersunk headed rivet, as used for attaching the outside plating to the stern and stern port. The countersunk heads extended right through the plates, and all the screws were cut with an ordinary Whitworth angular thread. The tests for each kind of rivet were made in duplicate, and the exact mean shearing stress per rivet obtained is shown in Appendix III., which also gives the net sectional area of the rivets, *i.e.*, their area to the root of the thread, and the shearing stress per square inch of that area. It will be seen that the shearing stress of the screw rivet is much less than for the hammered rivet of the same nominal size, as was, of course, to be expected. The shearing stress of the continuous screws differed but slightly from the screw pointed and hexagonal headed rivets; and it would appear to be correct to take the following units for the shearing stress of the different sized iron rivets of these kinds:—

$\frac{3}{4}$-inch rivet, 5·75 tons; $\frac{7}{8}$-inch rivet, 7·75 tons; 1-inch rivet, 10·25 tons.

The results for the countersunk headed and screw pointed iron rivets were superior to the others, and the following shearing stresses for this kind of rivet may be accepted:—

$\frac{3}{4}$-inch rivet, 6·75 tons; $\frac{7}{8}$-inch rivet, 9·25 tons; 1-inch rivet, 12·25 tons.

These stresses are practically in proportion to the net sectional area of the rivets. It may be desirable to add that in estimating the shearing stress of other sized rivets, from the units given above, allowance should be made for the fact that the depth of the Whitworth thread in relation to the diameter of the rivet is less for the larger rivets than the smaller.

Comparing the stresses for the iron screw rivets with the results obtained at Pembroke, in 1878, for ordinary hammered iron rivets in steel plates, we find that the continuous screw, and the screw pointed and hexagonal headed rivets, are about 30 per cent. weaker than the hammered rivets of the same nominal size; and that the countersunk headed and screw pointed rivets are about 17 per cent. weaker. The superiority attaching to these latter rivets, as compared with the other screws, may be due to increased friction between the plates and straps; but as the countersunk heads extended completely through the plates, the rivets were sheared at the top or end of the thread, and the sectional area there may possibly have been somewhat greater than has been taken account of, and if so, the shearing stress per square inch of these rivets would, of course, be slightly less than now stated.

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The stress per square inch of net sectional area of the iron screw rivets is in excess of the stress per inch for the hammered iron rivets in steel plates, from which it may be inferred that the weakness of the screw rivets, as compared with the hammered, is wholly due to the loss of sectional area from cutting the thread, and not from any loss of friction between the plates and straps.

After these tests with the iron rivets in steel plates, precisely similar tests were made, except that steel rivets were used instead of iron. The rivets were made from ordinary pan headed steel rivets, and there were eighteen tests altogether, *i.e.*, two for each size and kind of screw rivet. As in the case of the iron rivets, there was but little or no difference between the results for the continuous screws and the screw pointed and hexagonal headed rivets; and the results for the countersunk headed and screw pointed rivets were superior to the others. The mean results for the shearing stress per rivet, after allowing for accidental differences, were as follows:—

	¾-inch rivet. Tons.	⅞-inch rivet. Tons.	1-inch rivet. Tons.
Continuous screw, or hexagonal headed and screw pointed rivet	6·25	8·5	11·75
Countersunk headed and screw pointed rivet	7·25	9·75	13·5

These figures are practically in proportion to the net sectional area of the rivets, and on the whole, they are about 12 per cent. in excess of the results obtained with the iron rivets in steel plates.

We come now to the tests with the hammered steel rivets in the three sets before us. These rivets were of three kinds, namely, pan headed and countersunk pointed, countersunk headed and pointed, and pan headed and snap pointed; and there were twenty-six tests made in all. The sizes of the rivets and the particulars of the plates have already been stated.

Referring first to the pan headed and countersunk pointed rivets, four tests were made for each size of rivet, and the results of the experiments furnish the following shearing stresses per rivet as units for general guidance:—

¾-inch rivet.	⅞-inch rivet.	1-inch rivet.
11·5 tons.	15·25 tons.	20·25 tons.
	(15·28)	(19·65)

The figures in brackets are in exact proportion to the sectional area of the rivets, the stress for the ¾-inch rivet being taken as a basis. The result for this rivet agrees practically with that obtained from the experiments of 1880, but the stress for the ⅞-inch rivet is about half-a-ton greater. Half of these tests had the countersinking stopped $\frac{1}{16}$th of an

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inch short of the full thickness of the plates, and in the other half the countersinking extended right through; but the results were on the whole as good one way as the other, and there was but little variation in them individually.

Referring next to the countersunk headed and pointed rivets, it was found that the rivets of this form gave out more readily, in some cases, than the pan headed and countersunk pointed rivets, and without being sheared. In the tests with the $\frac{3}{4}$-inch and $\frac{7}{8}$-inch rivets, they were nearly all drawn through the holes, and no final shearing stress was obtained. The mean ultimate stress per rivet applied before the joints parted, was, however, just over 11 tons for the $\frac{3}{4}$-inch rivet, and 13·6 tons for the $\frac{7}{8}$-inch rivet. In the tests with the 1-inch rivets, which, it will be remembered, were used in $\frac{3}{4}$-inch plates, the rivets were sheared in all cases, and at a mean stress per rivet of 20·9 tons, which is a somewhat better result than for the pan headed and countersunk pointed rivet.

Referring lastly to the tests with the pan headed and snap pointed rivets, the results appear to show that this kind of riveting is not quite so strong as the pan headed and countersunk pointed riveting; and they furnish the following units of shearing stress per rivet for general guidance:—

$\frac{3}{4}$-inch rivet.	$\frac{7}{8}$-inch rivet.	1-inch rivet.
11·0 tons.	14·75 tons.	19·0 tons.
	(14·62.)	(18·79.)

The figures in brackets are, as before, in exact proportion to the net sectional area of the rivets, taking the $\frac{3}{4}$-inch rivet as the base.

Comparing the foregoing results for the hammered steel rivets with those previously given for the screw steel rivets, it appears that the continuous screw, and hexagonal headed and screw pointed rivets are about 44 per cent. weaker than the hammered countersunk rivets of the same nominal size; and the countersunk headed and screw pointed rivets are about 35 per cent. weaker. This shows the desirability in cases where the shearing stress is important, as, for example, in the connection of the outside plating to the stem in war ships, of continuing the body or shank of the rivet untapped for some distance in beyond where it might be sheared, to prevent this taking place at the parts already weakened by the thread.

The shearing stress of the hammered rivets per square inch of sectional area* (when finished) was practically identical for the different sizes, but was about $1\frac{1}{4}$ tons greater for

* In all cases the sectional area referred to is the area of the rivets when finished in the test plates, and not as obtained from their nominal diameter.

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the countersunk pointed rivets than for the snap pointed. The exact mean results from the tests were 22·6 tons per square inch of sectional area for the former rivets, and 21·4 tons for the latter. The tensile strength of the bars from which these rivets were made may be taken at 28 tons per square inch.

It may be interesting to compare these shearing stresses with those obtained by Mr. Kirkaldy for "machine" riveting in steel plates lap-jointed. Plates, $\frac{1}{4}$-inch to 1-inch thick, in steps of a quarter of an inch, were connected together with steel "machine" rivets from $\frac{7}{16}$-inch to $1\frac{1}{16}$-inch in diameter, the joints being treble riveted. The ultimate shearing stress per square inch of the rivets was found to be from 24·1 to 25·5 tons, their tensile stress being from 30·1 to 31·2 tons per square inch. The higher stresses were obtained with the smaller rivets. It will be seen that the shearing stress per square inch for the "machine" riveting is from $1\frac{1}{2}$ to $2\frac{3}{4}$ tons greater than for the countersunk "hand" riveting. But the shearing stress is, in each case, 20 per cent. less than the tensile stress stated, from which it would appear that "machine" riveting gives but little, if any, advantage over countersunk "hand" riveting, as regards the ultimate strength of the joint.

The shearing stress per square inch of sectional area of iron rivets when used in iron plates, taking 10 tons for the $\frac{3}{4}$-inch rivet as a basis, is about $19\frac{1}{4}$ tons; and, if we allow $8\frac{1}{2}$ tons say for the $\frac{3}{4}$-inch iron rivet in steel plates, their shearing stress per square inch of sectional area, when so used, is about $16\frac{1}{2}$ tons. The shearing stress per unit of area of pan headed and countersunk pointed steel rivets in steel plates is about 17 per cent. greater than for iron rivets in iron plates, while the tensile strength of the steel plates themselves is, as already stated, as much as 30 per cent. greater than for the iron plates. This indicates the necessity of increasing the relative number or size of the steel rivets, to secure sufficient strength in the rivet connections of the steel plates.

It now remains to consider the four cases with the treble rivetted straps, previously mentioned. In these tests the straps were fastened with $\frac{3}{4}$-in. pan headed and countersunk pointed steel rivets, and the mean shearing stress per rivet was found to be a little over $11\frac{3}{4}$ tons, which is a somewhat better result than obtained with the double riveted straps.

Hitherto, our attention has been directed to the single shear only of the rivets; we come now to a set of six experiments (see Table 6), to determine the double shear of steel rivets, such as would happen in drawing plates asunder when connected by a strap on each side. The plates used were $\frac{1}{2}$-in. thick, and were cut to the same general shape as before, with four rivets in each. The tests were duplicated, the plates and straps being connected

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with $\frac{5}{8}$-in. and $\frac{3}{4}$-in. pan headed and snap pointed rivets. The holes were punched in the plates, and drilled in the straps. In two cases the plate broke across the rivet holes, and at a mean stress of only 23·1 tons per square inch of sectional area. But from the other four tests, the mean double shear of the $\frac{5}{8}$-in. rivet was found to be 16·1 tons, and the double shear of the $\frac{3}{4}$-in. rivet, 21·2 tons. These figures are in exact proportion to the sectional area of the rivets, and they show that $21\frac{1}{4}$ tons may be taken as the unit for the double shear of a $\frac{3}{4}$-in. steel rivet in steel plates, the stress for other sized rivets being proportioned to their sectional area.

The exact mean results of the shearing stresses per rivet and per inch of sectional area, for the various sizes and kinds of steel rivets used in the experiments with the double riveted straps, are given in Appendix III.

The next set of experiments made had reference to the frictional resistance of riveted joints, and were in fact a repetition of experiments of a similar nature described in Sir E. J. Reed's work on "Shipbuilding in Iron and Steel." The shape of the plates, and the method of connecting them together, are shown on sketches E (see Table 7), from which it will appear that the middle plate had the material cut away opposite the rivets to allow it to slip from between the other two without shearing the rivets. Different kinds of rivets were employed, some being "hand" riveted and others "machine" riveted. The rivets were of steel, and were 1-in. and $\frac{3}{4}$-in. in diameter, the former being used in $\frac{3}{4}$-in. steel plates, and the latter in $\frac{1}{2}$-in. plates. The breadth of lap was three diameters, and the length of joint or lap was eleven diameters, the pitch of the rivets being four diameters. There were four kinds of "hand" rivets of each size, viz.: (1) snap headed and pointed; (2) pan headed and boiler or conical pointed; (3) pan headed and countersunk pointed; and (4) countersunk headed and pointed. The experiments were made in duplicate, and the mean frictional stress per rivet was found to be as follows:—

	1-in. rivet.	$\frac{3}{4}$-in. rivet.
With snap head and point	6·4 tons	4·72 tons.
With pan head and boiler point	7·36 „	4·52 „
With pan head and countersunk point	8·55 „	6·25 „
With countersunk head and point	9·04 „	4·95 „

These results, as in the earlier experiments, are not in proportion to the sectional area of the rivets, but they go to show that on the whole the friction is greatest for the countersunk rivets. The "machine" rivets had snap heads and points (when finished), and the mean friction per rivet was 9·6 tons for the 1-in. rivets, and 5·9 tons for the $\frac{3}{4}$-in. rivets.

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These results are much in advance of those for the snap, and boiler pointed, rivets, but they are not much, if at all, superior to those for the countersunk "hand" rivets.

The frictional stresses now furnished for the "hand" rivets are from 25 to 40 per cent. greater than given by Sir E. J. Reed for steel rivets in steel plates; those for the pan headed and countersunk pointed rivets, which in the earlier experiments were less than for the snap, and boiler pointed rivets, being specially satisfactory. The pan headed and countersunk pointed rivet is, as is well known, commonly used in the outside plating, upper deck, and other important parts of a ship's structure; and the experimental results now before us appear to indicate that the strength of this plating, as a whole, would not be greatly increased by resorting to "machine" riveting.

The last set of experiments to which we have to refer, were made with rolled steel strips of different thicknesses from ·2 to ·38 of an inch, and from 4¼ to 5* inches wide, as used for connecting the edges of flush-jointed plates. The tests were made to ascertain the ultimate tensile strength lengthwise and crosswise of the pieces selected, which were cut to the shape shown on sketches F. (see Table 8.) The results showed that the rolled strips were in every way as strong and suited for the purpose intended as strips cut from ordinary steel plates. There appears to be no objection to rolling steel butt straps to the breadth required, which would reduce their cost.

In conclusion, it may be observed that the experiments now put forward are not regarded as exhaustive. They leave some important questions untouched, and the tests relative to the strength of the steel plates were limited, as we have seen, to plates of but one thickness. The ultimate or breaking stress only of the plates and riveted joints has been considered, but the actual stresses ordinarily operating on a ship are, or ought to be, confined within the elastic limit of the material and connections. In the arrangements adopted for connecting the separate plates together in ships certain parts may possibly be made too strong or rigid, *i.e.*, more rigid than the surrounding material, which would subject those parts to an undue share of the strain borne by the whole. Experimental data for determining the stress at which permanent elongation begins, and the elastic stretch under varying stresses for joints riveted together in different ways, might prove instructive, especially in view of the great elastic strength of the steel plates themselves. More extended experiments as to the ultimate shearing stress of rivets might also be made, I think, with advantage. It is hoped, however, that the results now furnished will be found useful.

* Reduced in shaping to the breadth given in Table.

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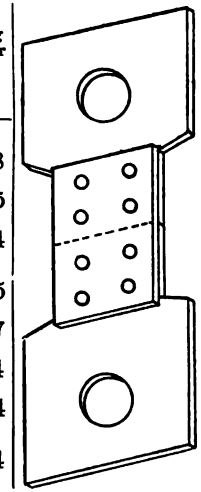
APPENDIX I.

Experiments made at Pembroke Yard in 1878, with Iron Rivets in Steel Plates.

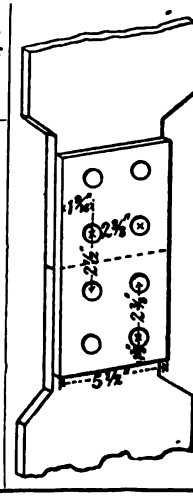
Temperature of House.	No of Experiment.	Description of Plate.	Nominal Size of Rivet.	Conditions of Test.	Shearing stress in Tons.	Shearing stress per rivet in Tons.
			Inches.	Rivets pan headed and countersunk pointed.		
56°	1	Landore Steel $\frac{9}{16}$-in. thick	¾	Holes countersunk right through plate . . .	33·273	8·318
,,	2	,, ,, ,,	¾	,, ,, ,, ...	32·420	8·105
,,	3	,, ,, ,,	¾	Holes countersunk to within $\frac{1}{16}$-in. of full thickness of plate	31·576	7·894
,,	4	,, ,, ,,	¾	,, ,, ,, ,, ,,	32·420	8·105
,,	5	Landore Steel $\frac{3}{4}$-in. thick	⅞	Holes countersunk right through plate . . .	47·588	11·897
,,	6	Bolton Steel $\frac{3}{4}$-in. thick .	⅞	,, ,, ,, ...	45·054	11·264
,,	7	,, ,, ,,	⅞	Holes countersunk to within $\frac{1}{16}$-in. of full thickness of plate	45·737	11·434
,,	8	Landore Steel $\frac{3}{4}$-in. thick	⅞	,, ,, ,, ,, ,,	45·737	11·434



Sketch of Test-piece.

Experiments made at Pembroke Yard in 1880, with Steel Rivets in Steel Plates.

Temperature of House.	No. of Test.	Description of Plate.	Nominal Size of Rivet.	Conditions of Test.	Shearing stress in Tons.	Shearing stress per rivet in Tons.
			Inches.	Rivets pan headed and countersunk pointed.		
55°	1	Landore Steel $\frac{9}{16}$-in. thick	¾	Holes countersunk right through plate . . .	45·908	11·477
,,	2	Landore Steel $\frac{3}{4}$-in. thick	¾	,, ,, ,, ...	46·249	11·562
,,	3	Landore Steel $\frac{9}{16}$-in. thick	¾	Holes Countersunk to within $\frac{1}{16}$-in. of full thickness of plate	46·932	11·733
,,	4	,, ,, ,,	¾	,, ,, ,, ,, ,,	47·102	11·775
54°	5	Landore Steel $\frac{3}{4}$-in. thick	⅞	Holes countersunk right through plate . . .	58·883	14·720
,,	6	,, ,, ,,	⅞	,, ,, ,, ...	60·261	15·065
,,	7	,, ,, ,,	⅞	Holes countersunk to within $\frac{1}{16}$-in. of full thickness of plate	58·029	14·507
,,	8	,, ,, ,,	⅞	,, ,, ,, ,, ,,	58·895	14·723



Sketch of Test-piece.

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APPENDIX II.

Table 1.—Tensile Tests with Steel Plates.

Temperature of House.	No. of Test.	Size of Test-piece at narrowed part. Breadth at holes (nett).	Thickness.	Area.	Breaking strain. Test-piece.	Per sq. in.	Elongation.	Sketch of Test-piece.	Remarks.
Fah.		Inches.	Inches.	Sq. ins.	Tons.	Tons.	Inches.		
58°	1	2·22	0·50	1·110	31·897	28·74	In 8 ins. 2·40	Parallel strips edges planed, as *A*.	Fracture of a fine silky appearance.
,,	2	2·21	0·50	1·105	31·808	28·78	In 8 ins. 2·35		,, ,,
,,	3	3·395	0·50	1·697	38·390	22·61	0·15	as *B*.	Holes punched $\frac{7}{8}$-inch.
,,	4	3·395	0·50	1·697	38·750	22·82	0·20		,, ,,
,,	5	3·055	0·50	1·527	46·091	30·17	0·35		Holes drilled $\frac{7}{8}$-inch and countersunk right through plate.
,,	6	3·10	0·49	1·519	46·772	30·79	In hole. 0·40		
,,	7	3·08	0·50	1·540	46·645	30·29	0·30		Holes punched $\frac{3}{4}$-inch, and increased by countersinking to $\frac{7}{8}$-inch.
,,	8	3·08	0·50	1·540	46·091	29·93	0·30		N.B.—The above tests were made from the same plate.
56°	9	2·21	0·48	1·061	29·240	27·56	In 8 ins. 2·20	as *A*.	Fracture of a fine silky appearance.
,,	10	2·21	0·48	1·061	29·572	27·87	In 8 ins. 2·20		,, ,,
,,	11	3·41	0·48	1·637	34·820	21·27	0·10	as *B*.	Holes punched $\frac{7}{8}$-inch.
,,	12	3·39	0·48	1·627	35·040	21·53	0·12		,, ,,
,,	13	3·43	0·48	1·646	46·428	28·20	In hole 0·29		Holes drilled $\frac{7}{8}$-inch.
,,	14	3·43	0·48	1·646	47·100	28·60	In hole 0·29		,, ,,
,,	15	3·03	0·48	1·454	39·060	26·85	0·30		Holes punched $\frac{3}{4}$-inch and increased by countersinking to $\frac{7}{8}$-inch.
,,	16	3·03	0·48	1·454	41·740	28·69	0·30		

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TABLE 2.—TENSILE TESTS WITH STEEL PLATES, CONNECTED BY A STRAP.

Temperature of House.	No. of Test.	Nominal Diameter of steel Rivet used in Test-piece.	Description of Rivet.	Size of Test-piece. Breadth at holes (nett).	Size of Test-piece. Thickness.	Size of Test-piece. Area.	Breaking strain of plate. Test-piece.	Breaking strain of plate. Per Sq. in.	Sketch of Test-piece.	Remarks.
Fah.		Inches.		Inches.	Inches.	Square in.	Tons.	Tons.		
58°	17	¾	Pan head and countersunk point	2·60	0·48	1·248	37·50	30·00	as *a*.	The whole of the Test-pieces from No. 9 to No. 30 inclusive were made from the same plate. The countersinking in 17, 18, 21 and 22 extended right through the full thickness of plate.
,,	18	,,	,, ,, ,,	,,	,,	,,	34·37	27·54		
,,	19	,,	Pan head and snap point . .	2·895	,,	1·390	34·82	25·05		
,,	20	,,	,, ,, . .	,,	,,	,,	34·82	25·05		
,,	21	⅞	Pan head and countersunk point	3·06	,,	1·469	42·86	29·18		
,,	22	,,	,, ,, ,,	,,	,,	,,	42·41	28·87		
,,	23	,,	Pan head and snap point .	3·40	,,	1·632	39·50	24·20		
,,	24	,,	,, ,, . .	,,	,,	,,	41·29	25·30		
55°	25	¾	,, ,, . .	3·35	,,	1·608	39·30	24·44	as *b*.	Spacing of Rivets [3½ dia.
,,	26	,,	,, ,, . .	,,	,,	,,	41·07	25·54		,, ,, ,,
,,	27	,,	,, ,, . .	3·72	,,	1·786	43·30	24·24		,, 4 ,,
,,	28	,,	,, ,, . .	,,	,,	,,	48·66	27·25		,, ,, ,,
,,	29	,,	,, ,, . .	4·18	,,	2·000	48·89	24·49		,, 4½ ,,
,,	30	,,	,, ,, . .	,,	,,	,,	48·21	24·10		,, ,, ,,

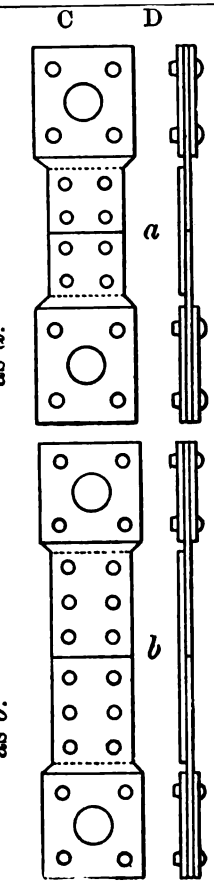

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TABLE 3.—SHEARING STRESS OF RIVETS IN STEEL PLATES.

Temperature of House.	No. of Test.	Nominal Dia. of Rivet.	Nature of Material of Rivet.	Description of Rivet.	Shearing Stress. Sample.	Shearing Stress. Per Rivet.	Remarks.
Fah.		Inches.			Tons.	Tons.	
55°	1	$\frac{3}{4}$	Iron	Continuous screw . . .	23·325	5·831	By continuous screw is meant screw having neither head nor clenched point.
,,	2	,,	,,	,, ,, . . .	22·098	5·524	
,,	3	,,	,,	Screw, hexagonal head . .	24·775	6·193	Screws tapped into strap only; free in test-plates.
,,	4	,,	,,	,, ,, . . .	24·664	6·166	
,,	5	,,	,,	Screw, countersunk head . .	26·785	6·696	Countersunk head extended right through plate.
,,	6	,,	,,	,, ,, . . .	25·781	6·445	
,,	7	,,	Steel	Continuous screw . . .	26·250	6·562	
,,	8	,,	,,	,, ,, . . .	25·670	6·147	Same as in 1 and 2.
,,	9	,,	,,	Screw, hexagonal head . .	26·678	6·669	
,,	10	,,	,,	,, ,, . . .	25·890	6·472	Same as 3 and 4 as to screws.
56°	11	,,	,,	Screw, countersunk head . .	28·785	7·196	Countersunk head extended right through plate.
,,	12	,,	,,	,, ,, . .	29·760	7·440	
,,	13	,,	,,	Pan head, countersunk point .	44·640	11·160	Countersinking extended to within $\frac{1}{16}$-inch of full thickness of plate.
,,	14	,,	,,	,, ,, .	44·866	11·216	
,,	15	,,	,,	,, ,, .	47·767	11·941	
,,	16	,,	,,	,, ,, .	48·430	12·107	Countersinking extended right through plate.
,,	17	,,	,,	Countersunk head and point .	No result obtained.		Rivets (No. 4) drawn through at 48·52 tons.
,,	18	,,	,,	,, ,, .	,,	,,	Rivets (No. 4) drawn through at 45·089 tons.
,,	19	,,	,,	Pan head and snap-point . .	43·300	10·825	
,,	20	,,	,,	,, ,, . . .	44·196	11·049	
60°	21	,,	,,	Pan head and countersunk point.	70·536	11·756	The countersinking in 17, 18, 21 to 24, inclusive, extended right through plate. In 22 the plate broke at *c d*.
,,	22	,,	,,	,, ,, .	No result obtained.		
,,	23	,,	,,	,, ,, .	75·000	12·500	
,,	24	,,	,,	,, ,, .	69·640	11·606	

Sketch of Test-plates and Strap.
Plates $\frac{1}{2}$ inch in thickness.

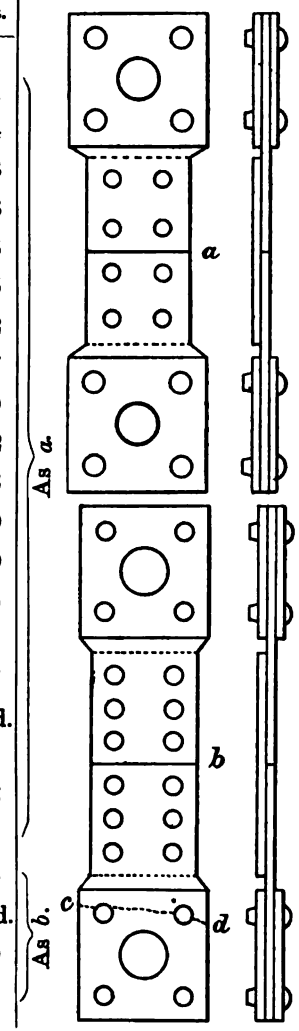

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TABLE 4.—SHEARING STRESS OF RIVETS IN STEEL PLATES.

Temperature of House.	No. of Test.	Nominal Dia. of Rivet.	Nature of Material of Rivet.	Description of Rivet.	Shearing Stress. Sample.	Shearing Stress. Per rivet.	Sketch.	Remarks.
Fah.		Inches.			Tons.	Tons.		
60°	25	$\frac{7}{8}$	Iron.	Continuous screw . . .	30·357	7·589		
,,	26	,,	,,	,, ,, . . .	30·401	7·600		
,,	27	,,	,,	Screw, hexagonal head . .	31·250	7·812	Plates $\frac{1}{2}$-inch in thickness.	Same as for corresponding tests in Table 3, as to screws and countersunk heads.
,,	28	,,	,,	,, ,, . . .	31·900	7·975		
,,	29	,,	,,	Screw, countersunk head . .	37·000	9·250		
,,	30	,,	,,	,, ,, . .	37·500	9·375		
,,	31	,,	Steel.	Continuous screw . . .	33·700	8·425		
,,	32	,,	,,	,, ,, . .	32·366	8·091		
,,	33	,,	,,	Screw, hexagonal head . .	34·642	8·660		
,,	34	,,	,,	,, ,, . . .	33·928	8·482		
,,	35	,,	,,	Screw, countersunk head . .	38·112	9·528		
70°	36	,,	,,	,, ,, . .	39·528	9·882		
,,	37	,,	,,	Pan head and countersunk point.	63·169	15·792		Countersinking extended to within $\frac{1}{16}$-inch of full thickness of plate.
,,	38	,,	,,	,, ,, ,, .	61·160	15·290		
,,	39	,,	,,	,, ,, ,, .	62·500	15·625		Countersinking extended right through plate.
,,	40	,,	,,	,, ,, ,, .	59·642	14·910		
60°	41	,,	,,	Countersunk head and point .	No result obtained.			Rivets (4 No.) drawn through, at 56·91 tons.
,,	42	,,	,,	,, ,, ,, .	,,			Two rivets sheared and two drawn through, at 54·01 tons.
,,	43	,,	,,	,, ,, ,, .	,,			One rivet sheared and three drawn through, at 53·571 tons.
,,	44	,,	,,	,, ,, ,, .	,,			Three rivets sheared and one drawn through, at 53·34 tons.
,,	45	,,	,,	Pan head and snap point . .	59·813	14·953		The countersinking in Nos. 41 to 44, inclusive, extended right through the plate.
,,	46	,,	,,	,, ,, ,, . .	59·150	14·787		

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TABLE 5.—SHEARING STRESS OF RIVETS IN STEEL PLATES.

Temperature of House.	No. of Test.	Nominal Dia. of Rivet.	Nature of Material of Rivet.	Description of Rivet.	Shearing Stress. Sample.	Shearing Stress. Per Rivet.	Sketch of Test-piece.	Remarks.
Fah.		Inches.			Tons.	Tons.		
60°	47	1	Iron.	Continuous screw	40·400	10·100	Plates $\frac{3}{4}$-inch thick.	Same as for corresponding tests in Table 3, as to screws and countersunk heads.
"	48	"	"	" "	40·178	10·044		
"	49	"	"	Screw, hexagonal head . .	39·760	9·940		The screws were in all cases cut with a Whitworth angular thread.
"	50	"	"	" " " . .	39·300	9·825		
"	51	"	"	Screw, countersunk head . .	50·440	12·610		
"	52	"	"	" " " . .	48·660	12·165		Diameter of 1-inch screw decimal 85 of an inch to inside of thread. Diameter of $\frac{7}{8}$-inch screw, decimal 72 of an inch to inside of thread. Diameter of $\frac{3}{4}$-inch screw decimal 62 of an inch to inside of thread.
"	53	"	Steel.	Continuous screw . . .	49·550	12·387		
"	54	"	"	" " . . .	49·640	12·410		
"	55	"	"	Screw, hexagonal head . .	47·320	11·830		
"	56	"	"	" " " . .	45·980	11·495		
58°	57	"	"	Screw, countersunk head . .	57·852	14·463		
"	58	"	"	" " " . .	56·760	14·190		
"	59	"	"	Pan head and countersunk point .	80·357	20·089		Countersinking extended to within $\frac{1}{16}$-inch of full thickness of plate.
"	60	"	"	" " " " .	87·946	21·986		
"	61	"	"	" " " " .	82·100	20·525		
"	62	"	"	" " " " .	77·900	19·475		Countersinking extended right through plate.
"	63	"	"	Countersunk head and point .	83·928	20·982		
"	64	"	"	" " "	83·480	20·870		
"	65	"	"	Pan head and snap point . .	77·680	19·420		
"	66	"	"	" " " .	75·446	18·861		

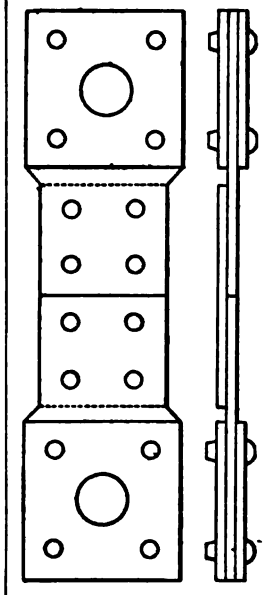

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TABLE 6.—DOUBLE SHEARING STRESS OF STEEL RIVETS IN STEEL PLATES.

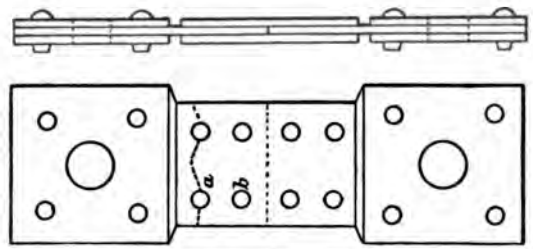
Temperature of House.	No. of Test.	Nominal Dia. of Rivet.	Description of Rivet.	Shearing Stress (Double). Sample.	Shearing Stress (Double). Per Rivet.	Sketch.	Remarks.
Fah. 60°	1	Inches. $\frac{5}{8}$	Pan head and snap point	Tons. 63·17	Tons. 15·79	Plates ½-inch thick. Holes punched in test plates and drilled in covers 	
,,	2	,,	,, ,, ,,	65·84	16·46		
,,	3	$\frac{3}{4}$	,, ,, ,,	No result obtained			Plate broke at 34·8 tons=22·9 tons per sq. in. of plate section.
,,	4	,,	,, ,, ,, ,,	,,	,,		Two rivets (a & b) sheared, and plate broke at 86 tons=22·3 tons per sq. in. of plate section.
,,	5	,,	,, ,, ,, ,,	80·80	20·20		
,,	6	,,	,, ,, ,, ,,	88·61	22·15		

TABLE 7.—RESULTS OF EXPERIMENTS WITH STEEL RIVETS IN STEEL PLATES, FOR THE PURPOSE OF DETERMINING THE FRICTIONAL RESISTANCE OF THE JOINTS.

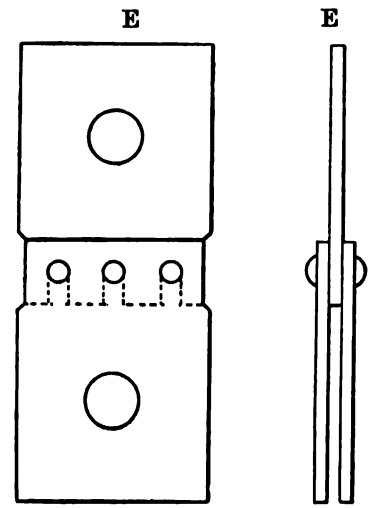
Description of Rivets.	"Hand" or "Machine" riveted.	Friction per Rivet. 1st Exp.	Friction per Rivet. 2nd Exp.	Friction per Rivet. Mean.	Nominal Dia. of Rivet.	Thickness of Plate.	Sketch of Test-piece.
Snap heads and points . . .	"Hand"	6·330	6·470	6·400	Inches. 1	Inches. $\frac{3}{4}$	
Pan heads and conical points . .	,,	7·440	7·291	7·365	,,	,,	
Pan heads and countersunk points .	,,	8·260	8·850	8·550	,,	,,	
Countersunk heads and points . .	,,	8·780	9·300	9·040	,,	,,	
Snap heads and points . . .	,,	4·687	4·761	4·724	$\frac{3}{4}$	$\frac{1}{2}$	
Pan heads and conical points . .	,,	4·017	5·030	4·523	,,	,,	
Pan heads and countersunk points .	,,	6·690	5·800	6·245	,,	,,	
Countersunk heads and points .	,,	4·613	5·282	4·947	,,	,,	
Snap heads and points (when finished)	"Machine"	8·183	11·900	10·041	1	$\frac{3}{4}$	
,, ,, ,, ,,	,,	10·341	7·932	9·136	,,	,,	
Snap heads and points (when finished)	,,	6·101	5·469	5·785	$\frac{3}{4}$	$\frac{1}{2}$	Length of lap 3 diameters Breadth ,, 11 ,, Pitch of rivets 4 ,,
,, ,, ,, ,,	,,	6·000	6·137	6·068	,,	,,	

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TABLE 8.—TESTS MADE WITH ROLLED STEEL STRIPS.

Temperature of House.	No. of Test.	Size of Test-piece.			Breaking Strain.		Elongation in 8 inches.	By whom supplied.	Sketch of Test-piece.	Remarks.
		Breadth.	Thickness.	Area.	Test-piece.	Per sq. in.				
Fah.		Inches.	Inches.	Sq. inches.	Tons.	Tons.				
70°	1 L	4·00	0·36	1·44	42·54	29·54	2·85	Landore Company . .	Lengthwise (L)	
,,	2 ,,	4·00	0·36	1·44	42·41	29·45	2·70	,,		
,,	3 C	1·30	0·36	·468	13·61	29·08	—	,,		
,,	4 ,,	1·30	0·36	·468	13·83	29.55	—	,,		
,,	5 L	4·02	0·24	·965	27·45	28·45	2·50	,,		
,,	6 ,,	4·02	0·24	·965	27·00	27·98	2·25	,,		
,,	7 C	1·19	0·24	·286	8·37	29·30	—	,,		The appearance of the fracture was in each case sound and good.
,,	8 ,,	1·19	0·24	·286	8·25	28·88	—	,,		
,,	9 L	4·01	0·38	1·524	37·94	24·89	3·00	Steel Company of Scotland		
,,	10 ,,	4·01	0·38	1·524	37·34	24·50	3·20	,,		
,,	11 C	1·35	0·38	·513	13·39	26·10	—	,,		
,,	12 ,,	1·35	0·38	·513	14·06	27·40	—	,,		
,,	13 L	4·01	0·20	·802	22·77	28·39	2·30	Butterley Company . .		
,,	14 ,,	4·01	0·20	·802	23·00	28·67	2·60	,,		
,,	15 C	1·12	0·20	·224	6·14	27·41	—	,,		
,,	16 ,,	1·12	0·20	·224	6·96	31·07	—	,,		
,,	17 L	4·06	0·28	1·137	31·47	27·68	2·50	Landore Company . .	Crosswise (C)	The elongation in the pieces tested crosswise could not be accurately measured in consequence of their distortion. Each piece was well reduced at the point of fracture.
,,	18 ,,	4·06	0·28	1·137	31·74	27·92	2·25	,,		
,,	19 C	1·40	0·28	·392	9·93	25·33	—	,,		
,,	20 ,,	1·40	0·28	·392	10·00	25·51	—	,,		
,,	21 L	4·03	0·23	·927	25·22	27·20	2·50	Butterley Company . .		
,,	22 ,,	4·03	0·23	·927	25·66	27·68	2·90	,,		
,,	23 C	1·30	0·23	·299	8·37	27·99	—	,,		
,,	24 ,,	1·30	0·23	·299	8·48	28·36	—	,,		

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APPENDIX III.

Riveting. Mean Results of Experiments with different kinds of Rivets in Steel Plates connected by single straps double riveted.

Material of Rivets.	Description of Riveting.	¾-inch rivets in ½-in. plates.			¾-inch rivets in ½-in. plates.			1-inch rivets in ¾-plates.		
		Net sectional area of rivet when finished	Mean shearing stress per rivet.	Mean shearing stress per square inch of sectional area of rivets.	Net sectional area of rivet when finished.	Mean shearing stress per rivet.	Mean shearing stress per square inch of sectional area of rivets.	Net sectional area of rivet when finished.	Mean shearing stress per rivet.	Mean shearing stress per square inch of sectional area o rivets
		Inches.	Tons.	Tons.	Inches.	Tons.	Tons.	Inches.	Tons.	Tons.
Iron	Continuous screw	·302	5·677	18·80	·407	7·594	18·70	·567	10·072	17·76
,,	Hexagonal head and screw point tapped into strap only	,,	6·179	20·47	,,	7·893	19·40	,,	9·883	17·41
,,	Countersunk head and screw point. Head countersunk right through plate, point tapped into strap	,,	6·570	21·76	,,	9·312	22·88	,,	12·387	21·85
Steel	Continuous screw	,,	6·354	21·04	,,	8·258	20·29	,,	12·398	21·86
,,	Hexagonal head and screw point tapped into strap only	,,	6·570	21·76	,,	8·571	21·05	,,	11·662	20·56
,,	Countersunk head and screw point. Head countersunk right through plate, point tapped into strap	,,	7·318	24·24	,,	9·705	23·84	,,	14·327	25·27
,,	Pan head and countersunk point. Countersink $\frac{1}{16}$-inch less than full thickness of plate	·519	11·188	21·56	·690	15·541	22·52	·887	21·037	23·72
,,	Do. except that countersink extended right through plate	,,	12·024	23·17	,,	15·267	22·13	,,	20·000	22·55
,,	Countersunk head and point . . .	,,	No result obtained. Rivets drawn through at a mean of 11·076 tons per rivet.		,,	No result obtained. Some rivets drawn through, others sheared. Breaking stress per rivet =13·61 tons.		,,	20·926	23·59
,,	Pan head and snap point	,,	10·937	21·07	,,	14·870	21·55	,,	19·140	21·58

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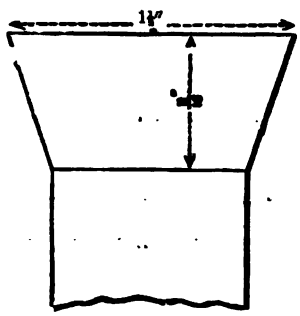
CODE OF TESTS FOR STEEL RIVETS.

1. The rivets are to be made in strict accordance with the dimensions given in the Schedule, and with the drawings hereunto annexed. Each rivet is to be marked in one place with the Broad Arrow ⋀. They are to be supplied of any lengths demanded.

Drawings of 1-*inch Rivets of the several descriptions specified in the Schedule herewith.*



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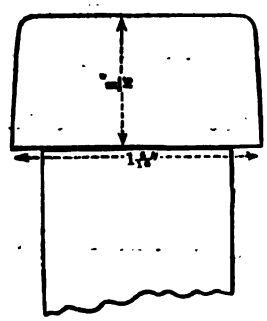

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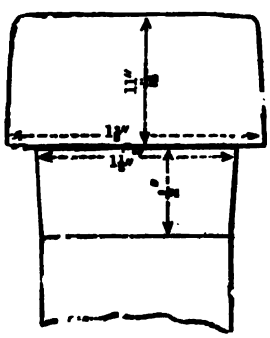

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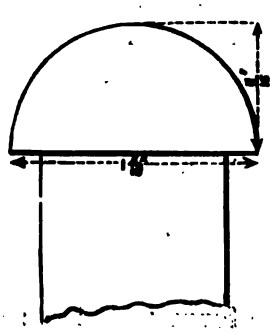

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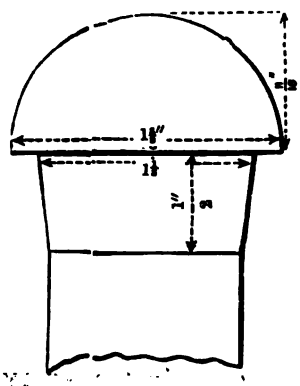

G

2. The rivets are to be made from steel bars having an ultimate tensile strength of not less than 26, and not more than 30 tons per square inch, with a minimum elongation

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of 20 per cent. in a length of 8 inches. A portion of one bar to be taken for testing from every 50, or portion of 50, before making into rivets.

Pieces cut from every bar, heated uniformly to a low cherry red, and cooled in water of 82° Fahrenheit, must stand bending in a press to a curve of which the inner radius is equal to the radius of the bar tested.

3. *Tests.*—The whole of the said rivets are to be properly heated in making, and care is to be taken that the finished rivets cool gradually. The rivets are to stand the following forge tests:—

(*a*) Bending cold without fracture in the manner shown in Figure 1 in the annexed diagram where the line A B equals one diameter of the rivet.

(*b*) Bending hot without fracture in the manner shown in Figure 2.

(*c*) Flattening of the rivet head, while hot, in the manner shown in Figure 3, without cracking at the edges. The head to be flattened until its diameter is 2½ times the diameter of the shank.

(*d*) The shank of the rivet to be nicked on one side, and bent over to show the quality of the material.

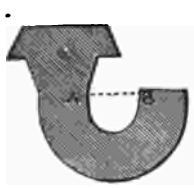

Fig. 1.

Fig. 2.

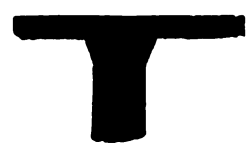

Fig. 3.

One rivet in every hundred to be forge tested as a sample.

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SCHEDULE OF DIMENSIONS.

Diameter of Rivet.	With Pan or Snap Head and Straight Necks to Drawing D or F.		With Countersunk Heads to Drawing A.		With Pan or Snap Heads and Conical Necks to Drawing E or G.			
	Diameter of Head.	Depth of Head.	Diameter of Head.	Depth of Head.	Diameter of Head.	Depth of Head.	Diameter under Head.	Depth of Cone.
Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.
$1\frac{3}{8}$	$1\frac{15}{16}$	1	—	—	2	1	$1\frac{1}{2}$	$\frac{5}{8}$
$1\frac{1}{4}$	$1\frac{11}{16}$	$\frac{7}{8}$	$1\frac{3}{4}$	$\frac{3}{4}$	$1\frac{3}{4}$	$\frac{7}{8}$	$1\frac{5}{8}$	$\frac{9}{16}$
$1\frac{1}{8}$	$1\frac{1}{2}$	$\frac{3}{4}$	$1\frac{5}{8}$	$\frac{3}{4}$	$1\frac{9}{16}$	$\frac{3}{4}$	$1\frac{1}{4}$	$\frac{1}{2}$
1	$1\frac{5}{16}$	$\frac{11}{16}$	$1\frac{1}{2}$	$\frac{11}{16}$	$1\frac{3}{8}$	$\frac{11}{16}$	$1\frac{1}{8}$	$\frac{1}{2}$
$\frac{7}{8}$	$1\frac{3}{16}$	$\frac{5}{8}$	$1\frac{3}{8}$	$\frac{5}{8}$	$1\frac{1}{4}$	$\frac{5}{8}$	$\frac{15}{16}$	$\frac{7}{16}$
$\frac{3}{4}$	$1\frac{1}{16}$	$\frac{9}{16}$	$1\frac{1}{8}$	$\frac{7}{16}$	$1\frac{1}{16}$	$\frac{9}{16}$	$\frac{13}{16}$	$\frac{3}{8}$
$\frac{5}{8}$	$\frac{7}{8}$	$\frac{1}{2}$	1	$\frac{5}{16}$	$\frac{7}{8}$	$\frac{1}{2}$	$\frac{11}{16}$	$\frac{5}{16}$
$\frac{1}{2}$	$\frac{3}{4}$	$\frac{3}{8}$	$\frac{3}{4}$	$\frac{1}{4}$	$\frac{3}{4}$	$\frac{3}{8}$	$\frac{9}{16}$	$\frac{1}{4}$
$\frac{7}{16}$	$\frac{11}{16}$	$\frac{5}{16}$	—	—	—	—	—	—
$\frac{3}{8}$	$\frac{5}{8}$	$\frac{5}{16}$	$\frac{5}{8}$	$\frac{1}{4}$	—	—	—	—
$\frac{5}{16}$	$\frac{1}{2}$	$\frac{1}{4}$	—	—	—	—	—	—
$\frac{1}{4}$	$\frac{7}{16}$	$\frac{3}{16}$	—	—	—	—	—	—
$\frac{3}{16}$	$\frac{3}{8}$	$\frac{1}{8}$	—	—	—	—	—	—

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SOME NOTES ON THE STRENGTH OF RIVETED JOINTS.

By J. T. Milton, Esq., Member.

[Read at the Twenty-sixth Session of the Institution of Naval Architects, March 27th, 1885; the Right Hon. the Earl of Ravensworth, President, in the Chair.]

In estimating the strength of the riveted joints of boiler shells, the usual method is to calculate the proportion which the section of plate left between the rivet holes bears to that of the solid plate, and to consider this as the strength of the plate at the joint, also to calculate the sectional area of rivets exposed to shear, and to ascertain their strength by crediting them with a certain strength per square inch to resist shearing; then by comparing the result with the assumed strength of the solid plate, and with the strength of plate at the holes as obtained above, the strength of the joint is estimated.

This method is only of value for comparing the strength of similar, or nearly similar joints, as in them it may fairly be assumed that the distribution of the stresses will be similar, and therefore their mean value will be nearly alike in all cases; but when we come to compare *dissimilar* joints by the same rules we shall be led astray, for it will in general happen that the distribution of the stresses will be sufficiently different to make a marked difference in the actual strength.

This method of examining the efficiency of riveted joints, by calculations of their sectional areas only, has led to the proposal of some forms of joints which have the appearance of possessing a large amount of strength because they have proportionally larger areas of material to be broken than ordinary riveted joints, but the superiority of some of them, in my opinion, is not so great as their advocates claim for them.

One very important point which is left out of consideration in these calculations, and to which I now wish to draw particular attention, is the elasticity, or stretch, or yielding of the material due to the forces acting upon it, as this yielding considerably modifies the distribution of the stress on the different parts of the joint, and especially upon the rivets.

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It is well known that materials when exposed to direct tension elongate in direct proportion to that tension up to the elastic limit, and that, beyond that limit, especially in the case of mild steel, the elongation is very considerable. The following figures, taken from a diagram obtained by an automatic recording machine, introduced for such purposes by Mr. J. H. Wicksteed, of Leeds, shew the uniform elongation which takes place with mild steel before local thinning of the test piece takes place:

Stress per square inch in tons.	Elongation of unit length per cent.
20	1
25	3·5
26	4·5
27	6·7
28	10·1
29	15·0
Ult. stress 29·5	Ult. elongation 25

Table 2, for which I am indebted to Professor Kennedy, of University College, London, shows clearly the deformation of one inch steel rivets exposed to a shearing strain.

MEMORANDUM OF TESTS OF SHEARING RESISTANCE OF RIVET STEEL.

Stress per square inch.	343	344	345	346	347	348
Lbs.						
0	0·0	0·0	0·0	0·0	0·0	0·0
6,365	·010	·013	·016	·022	·055	·021
12,730	·022	·028	·030	·034	·066	·032
19,100	·034	·040	·042	·048	.078	·043
25,460	·055	·060	·060	·071	·091	·062
28,320	·066	—	—	—	—	—
31,830	·080	·086	·082	·091	·113	·083
35,010	·093	—	—	—	—	—
38,190	·113	·113	·108	·114	·140	·108
41,380	·141	—	—	—	—	—
44,550	·168	·152	·142	·170	·171	·155
47,740	·200	—	—	—	—	—
50,910	·242	·200	·196	·248	·238	·222
54,110	—	—	—	—	—	—
Breaking Load per square inch {Pounds	54,110	54,930	55,240	52,830	56,666	53,530
Breaking Load per square inch {Tons	24·15	24·52	24·66	23·59	25·29	23·90

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Now let us consider the deformations of, first, an ordinary double-riveted lap joint; second, an ordinary treble-riveted lap joint; and, third, a quadruple-riveted lap joint.

In a double-riveted lap joint in which the plates connected are of equal thickness we see at once from symmetry that the stresses on each row of rivets are equal (Fig. 1, Plate XIV). The stress on the part of plate A B = that on part of plate *a b*, the elongations of these parts of the plate and the deformation of the rivets are therefore equal, and consequently the stresses on them are equal also.

Next, consider a treble-riveted lap joint with equal thicknesses of plate (Fig. 2, Plate XIV). In this case it is also evident from symmetry that the stresses on each of the outer rows of rivets are equal.

It is also evident at once that the force sustained by each of the portions of plate B C and *a b* is equal to the force sustained by one of the outer rows of rivets, while that sustained by each of the portions of plate A B and *b c* is equal to the sum of the forces sustained by one of the outer rows of rivets and by the inner row. A B is therefore subject to a greater stress and consequently to a greater elongation than *a b*, and *b c* similarly is more elongated than B C. It follows therefore that the deformation of each of the outer rows of rivets exceeds that of the inner row, by the difference of the elongations of *A B* and *a b*, or of *b c* and B C, caused by the forces acting on them. Further, as these elongations are proportional to the length of the plate elongated, it follows that the difference of the deformations of the rows of rivets will be greater the wider the lap of the plate is made.

Next, considering a quadruple-riveted joint (Fig. 3, Plate XIV), we see from symmetry, that each of the two inner rows is subjected to equal stresses, as also is each of the outer rows; but a similar reasoning to the foregoing will show that the outer rows are more severely strained than the inner rows, and as in this form of joint there is a greater proportion of rivet area exposed to less stress than in a treble-riveted joint, the efficiency of the joint, rivet area for rivet area, must be less. As before we see that the distribution of stress is more unequal the greater the distances A B and C D are made; but the portions of each plate between the two middle rows of rivets being equally strained, the joint is in just the same condition whether the distance B C is made long or short, and neglecting for a moment the weakening effect of the rivet holes, it would be the same if they moved up indefinitely near each other, that is, if they came into the condition of a treble riveted joint with the centre row of rivets at half pitch (Fig. 4, Plate XIV). But when we consider this joint we see that, as at the centre row the plate is weakened by the double number rivet holes, it must *yield more at the holes* than

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it would if pierced with the normal number of holes, and therefore this extra yielding at holes must increase the stretch of A B and *b c;* consequently this form of joint must be less efficient, so far as rivets are concerned, than the quadruple-riveted joint proper, with which it it usually compared, and hence still less efficient than the ordinary treble-riveted joint.

These considerations show that, even with joints having such close affinity as these different sorts of lap joints, it is unsafe to take one joint as a standard and to compare another form with it by the simple sectional area rule. Some actual experiments with riveted joints show clearly that practice bears out theory.

In 1878, some treble-riveted lap joints were made by Messrs. R. & W. Hawthorn, and tested by one of Lloyd's surveyors, the joints being samples of those proposed for one of the first steel marine boilers made by that firm.

The first joint was made with $\frac{3}{4}$-inch plates, 1-inch rivet holes pitched $4\frac{1}{8}$ apart centre from centre, the lap of the plate being $10\frac{1}{4}$ inches and the distance between the rows of rivets being $3\frac{5}{8}$ inches. The width of joint taken in all cases was such as to give three rivets in each row. This joint broke by shearing the rivets with a force of 157 tons, giving a mean strength of 22 tons per square inch. This result was unexpectedly low.

The second joint was then prepared, having the same pitch and lap, but the rivet holes increased to $1\frac{1}{8}$ inches diameter. This also broke by shearing the rivets at a force of 196 tons, giving again a mean shearing strength of 22 tons per square inch.

The third joint was then prepared with a reduced lap, the distance between the rows of rivets being $2\frac{3}{4}$ inches instead of $3\frac{5}{8}$. The rivet holes were slightly increased in diameter to give each exactly 1 square inch area, but the pitch of holes remained the same, and the same description of rivets were used as in last experiment. This joint broke by tearing the plate at a force of 225 tons, while the rivets, of precisely the same quality as those in the other joints, withstood a mean stress of 25 tons per square inch without breaking, thus showing that merely lessening the distance between the lines of rivets increased the average strength of the rivets (by rendering the stress more equally distributed among them) by more than 13 per cent.

Some lap joints made in 1879 by Messrs. Denny & Co., and tested by Mr. D. Kirkaldy, also broke by the shearing of the rivets. The plates were in each case $\frac{7}{8}$ths thick, the rivets

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$1\frac{1}{8}$ inch full, in diameter. In the first two joints there were three rows of rivets, the distance between the rows being $2\frac{3}{4}$ inches. The first joint broke at a mean stress of 19·4 tons per square inch on the rivets, and the second at a stress of 20·6 tons, giving a mean of 20 tons per square inch.

The third joint was made with four rows of rivets, the distance between the rows being reduced to two inches. The rivets in this sheared at a mean stress of 19·2 tons per square inch, showing that, although the rows were closer together, the disadvantage of four rows more than counterbalanced the effect of the closeness of the rows.

The mean tensile strength of the rivet steel in this case was found to be 28·9 tons per square inch, so that the actual shearing strength of the rivets could hardly have been less than 23 tons per square inch.

Next let us consider a butt-strapped joint. If the straps are each half the thickness of the plates, then, if they are double riveted, the stress on the plate between the rows of rivet holes is equal to that on the straps between the rows, and therefore the stretches of these portions will be equal; both rows of rivets will consequently be equally deformed and the stress on them will therefore be equal.

If, however, as is usually the case, the straps are thicker than this, then, since they will have a greater sectional area than the corresponding portion of the plate, and will be exposed only to the same force, they will stretch less, and therefore, as in a treble-riveted lap joint, the outer rows of rivets will be more deformed and consequently more strained than the inner row.

In this case, as in that of the treble-riveted lap joint, other things being equal, the rivets have the stress more equally divided between them the nearer the rows are placed to each other.

In a joint of this description made by Messrs. Denny and tested at the same time as the lap joints already quoted, and with the same description of rivets, the rivets sheared at 19·8 tons per square inch, but the joint was visibly strained at a very low stress.

In a treble-riveted butt-strapped joint (Fig. 5, Plate XIV.), if the straps are each half the thickness of the plate, the outside rows of rivets and the rows nearest the butt will be equally strained, each being more strained than the other rows; but if the straps are thicker than the plate, A will be more strained than B, owing to the less stretch

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of the straps between the rows A and B, while the strain on C will be lessened as the straps are increased in thickness, until at a thickness of each strap a little greater than the plate, the stretches of strap and plate between rows B and C will be equal, and these rows of rivets will be equally strained. The thickening of the straps, by decreasing the stress on the row C, relatively increases that on the row A, and therefore by rendering the stresses more unequally distributed between the rows, renders the rivets, area for area, less effective.

If either of the rows B or C are made with a double number of rivets, then, as the area of first row becomes a less proportion of the total area, it also becomes more intensely strained, for the same reason as the outer rows in the treble-riveted joint shown in Fig 4.

A joint has been put forward for boiler work shown in Fig. 6 (Plate XIV.), the wide strap being inside the boiler, it being considered that the pitch of the rivets in it may be made greater than would be consistent with tightness at high testing pressures, the outer strap having the rivets pitched at half the pitch of those in inner strap, giving a good caulking edge.

In this joint, if the straps are equal or nearly equal in thickness to the plate, we see that as the outer row of rivets contains only one-fifth of the total number of rivets, and these only in single shear while the other four-fifths are in double shear, the stress in A B of the strap must be only a small fraction of that in the plate, hence the plate must stretch much more than the strap, and the rivets in A must therefore be greatly deformed before any severe stress comes upon the other rivets. In my opinion, if a joint of this description is used for a boiler of ordinary scantlings, calculated on the per centage of area rules, it will be found that the hydraulic test will so strain the single shear rivets as to render it difficult to keep them tight, while I do not think the actual ultimate strength of the joint can be much more than that of the double-strapped portion which would be left if the extra width of inner strap were cut off.

Other forms of joints have been proposed, having a high per centage of sectional area; but in such joints it follows, that as at the first row of rivets the plate is little weakened, the rivet area in this row is small, and therefore the stretch of the covering plates is small, while that of the plate under them is great, so that the outer rows of rivets get most strained. As a general rule it will be found that the greater the per centage of areas in the joint the less uniformly will the stresses be distributed over the rivets, and therefore the less strength will they possess in proportion to their areas.

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I have thus far considered only the distribution of the stresses amongst the rivets. So far as the plate is concerned it only needs to be pointed out that if the stresses amongst the rivets be unequally distributed their pull upon the plates must be unequal, and therefore the stress on the plates cannot be so equally distributed as would be the case if the rivets all pulled alike. Professor Kennedy has shown in his experiments, conducted for the Institution of Mechanical Engineers, that with wide pitches, especially when the bearing pressures between the rivets and plate are great, the distribution of the stress across the plate is so unequal as to materially decrease its effective strength, so that as regards the plate section as well as the rivet sections it is not wise to estimate the strength of one form of joint from the known strength of another kind of joint merely from a comparison of their proportionate areas.

Although in this Paper I have ventured to criticise adversely the usual methods of estimating the strength of riveted joints from their sectional area, I do not wish it to be supposed that these methods are valueless. On the contrary, when they are employed solely for comparing *similar* joints they are undoubtedly of value, but it is their misapplication to joints of different proportions in which different conditions come in, that I wish to point out, especially, as at the present time, the tendency for excessive boiler pressure has been instrumental in causing several new proportions to be used.

What is required with these new forms of joint is some reliable experiments upon their strength by which to obtain *fresh data* by which they can be compared *amongst themselves* by a simple rule like the sectional area rule.

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DISCUSSION.

Professor ALEX. B. W. KENNEDY: My Lord, I think there can be no doubt that the hope which Mr. Wildish expressed at the end of his Paper, that his results would be found useful, will be amply fulfilled. His results will be no doubt as useful as they are unquestionably interesting. I hope I may not be considered egotistical, if I say that I feel sure he would find it interesting to look over the records of experiments which I myself have made for the Institution of Mechanical Engineers, on such joints as these. These experiments are published in their *Transactions*, and cover a great deal of the ground which he has gone over in his own Paper. As to these experiments, so far as they cover the same ground as that gone over by Mr. Wildish, I may say that we agree with each other in the main, but there are one or two points of difference. In the first place, as to the strength of punched holes in soft steel plate, I certainly found in very soft plate the punching in many cases did not diminish the strength, whereas the drilling of a hole increased the strength greatly more in proportion than Mr. Wildish's experiments have shown, often 10 per cent., and sometimes even 15 per cent. and more. This result refers not only to thin plates, but has been borne out equally by experiments on broad joints in three-quarter plates. This, of course, bears out Mr. Wildish's figures, but it carries them considerably further than he does. Apparently the excess of strength in the narrow neck of plate, between the holes, depends greatly on the proportion borne by the pitch of the holes to their diameter. In some cases where the rivets have been spaced very closely together, so as to make the joint apparently a very weak one, the proportional strength of the neck between the rivets has been so much increased by its narrowness, that the joint has actually turned out to have as great an efficiency as one which looked very much better. As to the strength of the rivets, which Mr. Wildish says much about, I noticed that at first he gave the strength *per rivet*, but finally, what appears to be the much more convenient plan, *per square inch of rivet*. I should like to point out, however, that the resistance measured per square inch of the nominal or actual area of the rivet, *cold*, cannot possibly be right. The diameter of the hole is always greater than that of the rivet, and if the rivet is properly closed up, it will probably fill the hole, so that the shearing area ought to be taken as *the area of the hole*, not the area of the rivet itself. In my own experiments I have measured the holes before riveting up, and have taken that as being the area of the rivets, and I have found after experiment, every indication that the rivet had actually filled the hole, and it was the actual area filled with steel or iron that was being shorn, and not the nominal area of the rivet.

Mr. J. G. WILDISH: It is not the nominal area given, it is the riveting area when finished.

Professor KENNEDY: I beg pardon, that is not stated, so I had assumed it was the area corresponding to the rivet diameter.

Mr. WILDISH: I think you will find that it is stated.

Professor KENNEDY: Then I am sorry I had not noticed it. If the area assumed is that of the hole, and not of the rivet before being put in, then we are agreed in this matter as in most others. On the question of machine *versus* hand riveting, the Research Committee of the Institution of Mechanical

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Engineers, for whom I have been working, have just finished a series of experiments on this. We made out in the result, that the ultimate strength of the machine riveting was certainly, as Mr. Wildish says, not greater than that of the hand riveting, the hand riveting being very good. But there was this very remarkable difference between them, that the point at which the riveting began to give way, the point at which you could see the *slip* by careful measurement, was quite double as great with machine riveting as with hand riveting. For practical purposes that is a point of even more importance than the other. I believe that that point is proportional probably to the point at which a boiler or a ship would begin to leak, and I believe therefore, that a hydraulic riveted joint would stand twice the stress before it would begin to leak than the hand riveted joint would, although their ultimate strengths would be about the same.

Perhaps Mr. Wildish will say, with regard to what is stated on page 190 of his Paper, whether the holes referred to were punched or drilled. I mean the holes in Series E.

As to Mr. Milton's Paper, I think it shows us once more, that we have gentlemen connected with this Institution and connected with a public body like Lloyd's, who can take, not only a practical interest in their work, but a very great and well directed interest in its scientific side. We may all congratulate ourselves on finding a man who is not content with looking at the mere outsides of things, and surface of his work, but who rather tries to see fairly into it and under it, especially when his theoretical investigations are so well directed, as in this case, as to become of practical value almost immediately. The matter which has been brought up by Mr. Milton, is not entirely new to me, but I must say, he has put it in a very forcible light, and in a much better way than I have ever put it for myself. I shall be very shortly making some large experiments on three-quarter and one-inch plates for the Mechanical Engineers, and will endeavour by consultation with Mr. Milton, to see if I can verify any of his results experimentally. I may say, however, that I should like to direct his attention to one point, which appears to me to present a little difficulty. Referring to his figure 2, the horizontal distance between the point A and *a*, that is to say, the distance that the one plate moves relatively to the other, is made up of two parts. One part is due to the squeezing of the rivet, and the other to the difference between the extensions of the plate, that is the difference between the the distances A B and *a b*. Now, unless I am wrong, I make out something like this. Let us suppose that in any joint we have a stress of 5,000 lbs. per square inch on one plate, and 2,500 per square inch on the other, which is just half. The one plate will stretch under that stress—I take it as three inches long between the rivets—·00045 of an inch, the other about ·00022, that is to say, of course, very small quantities, but at the same time, the rivet, if we suppose it to be carrying a shearing stress of 5,000 pounds to the square inch, would give about 1-100th of an inch. The difference therefore, between A B and *a b*, would seem to be almost all due to the "give" of the rivet, only $\frac{0{\cdot}00045-0{\cdot}00022}{0{\cdot}01}$, or about 2 per cent. of it, being actually due to the difference between the stresses on the plates emphasised by Mr. Milton. In this case it is difficult to think that this 2 per cent. can really make a very large difference in the strength of the joint, but will not rather be swallowed up and equalised in the larger distortion. I may, of course, be wrong here, but that is the way I have worked it out, and I give the figures for what they are worth. There is no doubt the rivets do give in themselves quantities which are very large in proportion to the very small extensions of the plates in ordinary joints. When we come to joints like figure 6, with exceptionally large differences, and exceptionally wide laps,

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the point mentioned by Mr. Milton, comes into more practical importance. I do not make these remarks with any wish to depreciate or to detract from the importance of what Mr. Milton has said, but merely because probably its practical importance in any case depends upon figures. If only the material of a joint would remain elastic until fractured, the question would be a very simple one, which could be decided by working it out arithmetically from known quantities, as to the known extensibility of the plates, and the known give of rivets, and so forth. In reference to the table at the top of page 205, it is a very convenient thing to remember, and is not always known, that the elongation of steel or iron is proportional to a very simple constant which one can always carry in one's head, namely, that (within the limits of elasticity) the elongation of a piece of steel or iron 10 inches long, under a stress of 1,000 pounds per square inch, is 0·3 of $\frac{1}{1000}$ of an inch. It is easy to remember that 0·3 (of a thousandth of an inch) is the extension at 1,000 pounds per square inch in a length of 10 inches, and this enables one to find very quickly the extension of a piece of any length, and under any stress, within its limit of elasticity. I have called the quantity just given, the *specific extension* of a material.

Mr. G. W. Manuel: My Lord and Gentlemen, we have to thank Mr. Wildish very much for his very able Paper on the subject of riveted joints, and the comparative stress of rivets of various forms. There is one remark I should like to make with regard to the construction of test pieces, as shewn in the drawings in Mr. Wildish's Paper, as I do not think they are constructed to give fair results to manufacturers of steel and shipbuilders, as far as my experience goes.

Taking the breaking strain of the specimen marked B, page 193, it would be much lower than in the plate when riveted up in a vessel, and the value or comparison between different made holes should not be decided by specimens such as that, but by a specimen having a series of holes or rivets fairly representing the whole breadth of plate. Therefore, I think, in making test specimens such as this, an extra allowance should be made on the narrow space between the outside edge of hole and the edge of the plate so as to give a fair result. Ships' plates, as a rule, are punched, and not annealed afterwards.

Again, I think on looking at the specimen test-piece in the paper marked C, page 194, that is a form of specimen which is unfair to the manufacturer or shipbuilder, for this reason, it is too short, and there is a notch at the edge of joint caused by the acute angle, where it joins the straight part, the latter should be made much longer, and the angle done away with altogether, with additional piece added to the edge of hole. In this specimen the plate broke through the first two holes next the acute angle, and *sheared* two rivets. A specimen made like this is unfair, for unless the strain was exactly in a line with the large holes and centre of joint, the acute angle would cause either the first rivet to shear or tear the plate through, commencing at the narrow edge next the rivet hole. I think you cannot depend on results from a specimen like this.

Mr. J. D'A. Samuda: It broke across the holes, did it not?

Mr. Manuel: It is shewn broken across, but I think it tore in this case from the edge, and the fact that it sheared two rivets in line shews this.

Mr. Wildish: My Lord, may I be permitted to say that these diagrams (B and C) are, I fear, rather misleading, inasmuch as they are not drawn exactly to scale, and are merely intended to show

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roughly where the two holes were made in the cross section at the narrowed part of the plates. The holes were, I think, a little closer together than they are shown in these diagrams. As regards the results obtained from the experiments, those with the drilled holes showed that the strength of the plates across where they broke was (at the holes) quite as much as could be expected, being, in fact, rather in excess per unit of area than for the other parts of the plates.

Mr. Manuel: I am inclined to differ from Mr. Wildish in his opinions. As the question of proper form of these test-pieces was brought prominently before me, and it was my duty to condemn plates or even boilers by the results of these test-pieces, such as these shewn, and I found that in order to be fair to the manufacturer and boilermaker I had to make fair specimen test-pieces, which were approved by the Committee of Lloyd's, on the allowances made so as to represent as near as possible the actual plate in the boiler. I do not think any allowance has been made in these specimens as shewn.

Mr. Wildish: You will observe in this diagram (C) a strengthening plate was put on each side of the test-plates so as to support the plates where they took the bearing, and distribute the strain by means of the rivets in the corners of the strengthening plates.

The President: I should point out to the Meeting that this is an inconvenient method of carrying on the discussion, because it renders it very difficult, if not impossible, to take it down properly, this being a sort of argument between two gentlemen over a diagram. I think, Sir, if you will go on with your speech it will probably be better.

Mr. Manuel: With regard to Mr. Milton's Paper, we have to thank that gentleman very much for bringing this subject again before us in this practical form.

It so happened that the specimens he refers to (in his Paper, page 207 of three test-pieces, representing the longitudinal shell-seams of a boiler constructing, in 1878, by Messrs. R. & W. Hawthorn) were designed and tested by me. I must say I was then put rather in a fix, for at that time the value of the shearing stress of steel rivets, compared with tensile strength of steel plates or rivets had not been carefully gone into, that is to say, it was not then known that the shearing stress of the steel rivets was only 18 tons, when the tensile strength of the plate or rivet was 28 tons. The plan of longitudinal joint had been approved in the usual way by the Committee of Lloyd's, and I was placed rather in a corner, the boiler shell being bolted up ready for drilling some of the holes to be used in the longitudinal seams, the actual test specimen failed at 157 tons, giving a percentage of rivet to plate of 60 per cent. shearing of the rivets at a low stress, and though Messrs. R. & W. Hawthorn had carried out all the requirements the boiler could not be passed. I did not at first know what to do, but arranged that the rivets, being the weak parts, should be made larger by one-eighth of an inch. Keeping the pitch the same, this second specimen joint with same quality of rivets also gave out by shearing rivets with a stress of 196 tons, giving a percentage of 74 per cent. to that of the solid plate. This was not considered enough, and I thought that more could be done, when a third specimen was made with rivets very slightly larger pitch; longitudinally the same, but as Mr. Milton says, the distance between the rows of rivets was reduced about one inch, also reducing the lap about two inches. I consider that each rivet has a certain binding strain keeping the plates surface to surface, therefore if you have a lap joint, say of

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ten inches, like this, and so many tons on these ten inches (owing to the pressure of the rivets), when you reduce it to eight inches lap, the pressure remaining the same, but being borne on a smaller area, gives a greater pressure per square inch, thereby adding more strength to the joint not by, as Mr. Milton says, a more "equal distribution," but by a greater *concentration* of the rivet strength.

Gentlemen, it so happened that this third specimen joint gave out by breaking the plate and shearing one of the rivets at the same time, and you may test a great many joints without getting the same results. The stress was 255 tons, and per centage of rivets to solid plate 85 per cent. These results were only gained by practical experience at a time when we had not the information we now possess. I am pleased Mr. Milton has mentioned these experiments, though the increased value he mentions was not gained by equal distribution, but by getting the power on a smaller area.

Before leaving this subject I should like to state that while great value must be placed on the tensile strength of a riveted joint, whether it is in a ship's plate or that of a boiler, we must not forget that the *life* of a boiler or ship or its *destruction* is very rarely due to any giving way of a joint as a whole through stresses which come on it, but more often due to leakage passing through these joints or seams.

When shipbuilders and boilerbuilders are constructing joints which will have the highest power when tested cold in specimens such as we have seen on drawings as these before us, they should bear in mind that the joint should be also made to withstand the greatest amount of *effort* to leak.

We must not be content that a joint will stand so many tons per square inch of section, when we find another that will not stand near so much will, by more attention to get tightness as well as strength, be the most durable and safe. When joints of boilers or vessels are constructed in this manner, I have endeavoured to point out, there will be less danger, and more profit to owners.

Mr. B. Martell: My Lord, I think we must all feel that we are greatly indebted to Mr. Wildish for the valuable information which he has brought before us in his Paper. You may remember, some year or two ago, it was suggested that the Admiralty would be doing good service to the shipping community if they would carry on a series of experiments like this, in view of the facilities they have for doing it. I do not know, but I suppose I am correct in assuming that this is somewhat the outcome of that suggestion. I think we are greatly indebted to the Admiralty, as well as to Mr. Wildish, for allowing him to bring these results before us, and I only hope, as suggested by Mr. Wildish, that an extension of these experiments will be made by the Admiralty, because such experiments as these are, no doubt, exceedingly useful to everyone who is interested in shipbuilding.

With regard to Mr. Milton's Paper, I cannot help feeling (I must be allowed to say this is almost the first time I have seen it) that Mr. Milton has opened out a new train of thought on this subject altogether. I think it throws great light upon the diversities between the results of experiments which have been made on riveted butts. We have in many instances, in making the experiments, found unaccountably that two butts which were secured in the same manner with similar butt straps, and with the material of a similar quality, have exhibited very different results; and I believe, if we look at the matter from the point of view that Mr. Milton has placed before us, it will account, to a very great extent, for the divergences in the results. But I must look at it myself in this Paper, from a naval architect's point of view, and not as an engineering one, so far as boilers are concerned, and I

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should not like it to go abroad that the rivet area rule upon which we have framed our regulations for the riveting of ships' plates is altogether wrong, or that we should be compelled, in arranging that rivet area, to take the very low strength that is shown by Mr. Milton here, of 19 tons, according to these experiments; because we must bear in mind that the working stresses to which ships are subjected come within the limits of elasticity, and that this extension is really not brought into play. With regard to the ordinary strains to which ships are subjected, that is the case; and, further than that, these butts are in every case succoured by the plating above and below, thus diminishing the inequalities in extension very materially indeed. It is exceedingly useful to have it brought out here. I have never looked at it from this point of view myself. I think this Paper shows a great deal of intelligence on the part of Mr. Milton, and that it reflects the highest credit upon him, as he is looking at it from quite a new point of view. But although this unequal straining of the material in this way must not be ignored, still, as regards the ordinary rules we adopt in riveting, I do not think it will make practically a great deal of difference in the rivet arrangements which one is accustomed to adopt at the present time. I do not say this in the least degree in disparagement of this Paper, because I think it is a most valuable one, and I think the greatest thanks are due to Mr. Milton for putting this subject before us in this form. It is a most valuable Paper; but I am bound to say, from a naval architect's point of view, the effect of this Paper, whatever it may be upon boilers, from an engineering point of view, will not make a very great deal of practical difference to us as far as regards the ordinary rivet area rule which we have been accustomed hitherto to adopt.

Sir John Dalrymple Hay: My Lord, with reference to what has fallen from Professor Kennedy, and also to Mr. Wildish's Paper, I should be glad to ask a question before he replies. Sir Edward Reed will remember—I think he has published it in his book on Shipbuilding in iron and steel, certain deductions made by the late Sir William Fairbairn on this subject some twenty-three or twenty-four years ago. At the bottom of pages 182—3 of the Paper which Mr. Wildish has just read there is an allusion made to the question of the weakening of the plates by punching. It was then ascertained, and I think those experiments were both upon steel and iron plates, that time entered very largely into the question of the strength of the plate, that where the bolster or die fitted accurately to the plug pushed out by the punching machine—when that plug was pushed out by a violent blow, great disintegration took place in the plate—where it was done by a thrust, there was less damage, and if it was pushed out as slowly as possible, hardly any weakening took place in the plate at all. I do not think Mr. Wildish has alluded to that matter in regard to the strength of plates. I, myself, in more recent years, looking on occasionally when the machines were at work, have seen the punching process going on with great rapidity; with great benefit as regards economy of time, but I think with some danger as to the weakening of the plates. I think if the pressure were more slowly brought to bear, and the bolster accurately fitted, or as nearly as possible, the plug that was to be pushed out, if the block or bolster itself was in rigid contact with the plate on which the punching took place, and if the punching was carried out as slowly as possible, less disintegration and weakening of the plate would occur. The experiments to which I have alluded, and which Sir Edward Reed has published in his book, which has been referred to in this Paper, were not alluded to by Mr. Wildish, and I thought, perhaps, hearing

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that Professor Kennedy was about to carry out other experiments on the matter, I might be permitted to refer to it.

Sir EDWARD REED: My Lord and Gentlemen, I should like to say one or two words upon the Papers which have been read. I think the discussion has illustrated the advantage of taking them together, and for this reason, when I heard Mr. Wildish's Paper read, I could not help feeling it was a pity that a Paper should be read and discussed without any reference to those valuable and most elaborate experiments which have been carried out practically by Professor Kennedy, under the authority of the Institution of Mechanical Engineers. I have felt for some time past that some member of this Institution would do very good service if he would bring together with ability and care the outcome of all these various experiments which have been going on in riveting for many years past. I feel with Mr. Martell that we are very much indebted not only to Mr. Wildish for his Paper, but to Mr. Barnaby and other Admiralty officers for allowing these results of Admiralty experiments to be brought before us. The Institution will remember that we had a Paper read on the first day by Mr. Watts which could not have been produced, but with the assent of the constructors for the Navy and of the Admiralty. With regard to Mr. Milton's Paper, as well as Mr. Wildish's, it carries my mind back to a time when Mr. White was doing some very valuable service in his investigations on this subject at the Admiralty, much of which took its place in my work on Shipbuilding in Iron and Steel. I had the advantage of having much assistance from Mr. White in collating the results of the experiments made in different dockyards, and bringing them into shape. Remembering that, I cannot entirely confirm the impression that Mr. Milton's Paper opens up an entirely new sphere of thought, because I think (Mr. White will correct me if I am wrong) the mode of viewing the subject which Mr. Milton has put forward was very frequently resorted to in the trials which we were then carrying out, and played a considerable part in the results at which we arrived. At the same time I feel very sensible myself of the value of checking, not only the results of experiments, but also the experiments made, by the more mature consideration of what must appear from the philosophical deductions which Mr. Milton has brought before us in his Paper. I myself feel—and that is the principal object I have in rising to address the meeting—that it would be some advantage to know that any further experiments which may be carried out by the Admiralty will be carried out with reference to what has been done under the auspices of the Institution of Mechanical Engineers, by Professor Kennedy, and I may say by other experimenters and investigators. The records of the transactions of the Scientific Institutions show a very large number of experiments of late years, both at home and abroad, in riveting, and I feel that we cannot have a proper knowledge of this question brought up to date without a survey of the whole experimental ground which has been previously covered. I do not know whether there is any Member of this Institution who would volunteer to make that survey, and collate the results of it; but I am quite sure if he would do so it would be of enormous advantage to the Members of the Institution, and not only so but to the advantage of naval architects and of marine engineers generally.

Mr. H. H. WEST: My Lord and Gentlemen, as Mr. Wildish has mentioned my name at the commencement of his Paper, I may perhaps be excused if I detain the meeting for a few minutes whilst I make two or three remarks. But before I say anything in connection with Mr. Wildish's Paper, I should like,

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if you would allow me, to go a little out of order to thank Mr. Milton for his Paper, to which I attach a great deal of value. This view of the subject—the deformation of the joint—was suggested to me in a conversation I had recently with a gentleman at Liverpool, with whom I was discussing the matter; but although it was suggested, I have never heard it advanced with the completeness and clearness with which Mr. Milton has put it forward to-day. There are points in Mr. Milton's Paper which I am afraid I have not sufficiently followed to speak confidently upon; but I find a little difficulty in accepting his conclusion unless he knows something that apparently is not stated in the Paper. He says: "The third joint was made with four rows of rivets, the distance between the rows being reduced to two inches. The rivets in this sheared at a mean stress of 19·2 tons per square inch, showing that, although the rows were closer together, the disadvantage of four rows more than counterbalanced the effect of the closeness of the rows." I want to know whether Mr. Milton has anything to tell him absolutely that it was solely the disadvantage of four rows which caused this joint to yield at practically the same mean stress per square inch as the first joint of the series, or whether it is simply an assumption? There might have been other circumstances in the testing of the latter joint which do not appear upon the Paper, which would account for the lower shearing stress. In passing to Mr. Wildish's Paper, I am very glad to see such an emphatic support of the use of steel instead of iron rivets in steel ships. This is a point upon which I have spoken before in this Institution, and I will not detain the meeting longer than simply to say I am very glad to see that Mr. Wildish's experiments now so very completely confirm what I had expected. Mr. Wildish makes a remark here on machine riveting. He says: "The shearing stress is in each case 20 per cent. less than the tensile stress stated, from which it would appear that machine riveting gives but little, if any, advantage over the countersunk hand riveting as regards the ultimate strength of the joint." Now, I have advocated in this room machine riveting so far as it is applicable to shipbuilding. I know it presents very great difficulties, and I know that a good many of those difficulties hitherto have been insuperable; but I believe they will not eventually be found insuperable. Be that as it may, the advantage I expect to be gained from machine riveting is this—That what we may call—for want of a better term—the "initial" slip of the joint will be reduced; that is to say, the holes will be better filled, and that part of the slip of the joint that accrues before a full bearing of the plate upon the rivet is attained, will be reduced materially by machine riveting. Not only that, but, as Professor Kennedy has pointed out in his very able report to the Institution of Mechanical Engineers, the actual amount of resistance to slipping—the frictional resistance of the joint—is very materially increased. It is double in machine riveting what it is in hand-riveting. If this were the proper place to discuss the transactions of another society, I would say that I do not think the difficulty that Professor Kennedy finds in accounting for the similarity of the frictional resistance of the $\frac{3}{8}$th joint and the $\frac{3}{4}$th joint is one that cannot be overcome. But we will let that pass for the moment. I come now to a point which was really the essential point of my Paper of last year. The question of the resistance of a joint to slipping under, not simply a dead direct strain in one direction only, but under an alternating strain. Sir Edward Reed has referred to the large number of memoirs and records of riveting experiments which exist in this and other countries. I have searched through many of these, and have written to French investigators, but I cannot find any record anywhere of a single experiment in alternating strains; that is, strains alternating in direction. Now, if we admit that the strains in a ship vary in direction, sometimes in one direction and sometimes in another; or even if we

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admit that they vary in intensity at different times, I think we at once set up a motion in the joint which must produce slackness in the riveting, if it will move at all—it is only a question of time—and you eventually get a joint that yields. A joint that yields under strain, permits by its movement a strain upon the surrounding material that it is not calculated for, and is not fitted to bear. I think, therefore, that before we discuss the ultimate strength of the joint, the primary consideration is, what is the strength of a joint at which it will yield to such an extent as to throw an undue strain upon the adjoining plates. In my Paper last year I gave a number of illustrations of riveted work in the top of a ship having yielded to such an extent that the adjoining plates had torn. At that time I was not in a position to say that I had ever seen any such working in the bottom of a ship. I have, however, seen a very remarkable instance of it quite recently in a large cargo-carrying steamer. This was a thoroughly good ship. I think no one could entertain a doubt on that point, though she was not classed in the Liverpool Registry. She had been subjected to very exceptionally heavy weather, and had suffered severely. There were certain damages and indications of severe strain on the upper works; but what I particularly wish now to draw your attention to is the bottom. About amidships we found several slack rivets in the keel, not scattered here and there, but for a distance of about six feet every rivet slack. My friend and colleague, Mr. Hamilton, examined the ship internally and went over it very carefully. In the butt of the garboard strake nearest to the slack keel rivets he found all the rivets on one side of the butt slack, and all the rivets on the other side perfectly sound. This filled him with concern, and he proceeded to make an examination of the next butt, and again he found all the rivets on one side of it slack, and all on the other side sound. On further examination he found that this condition of things was not confined to the garboard strake, but extended over the butts of the whole flat of bottom, in the neighbourhood of the place where the keel rivets were started, dying out towards the bilge, where, although the butts were showing more or less outside, the riveting apparently had not yielded. Now, this was clearly no question of defective workmanship, for bad workmanship would have been as likely to exist on one side of a butt as on the other. The remarkable feature of this case was that in a large number of butts of the bottom the whole of the riveting on one side of the butt was slack, while the whole of the riveting on the other was sound. I do not suppose that that ship was strained to such an extent, or anything like the extent, that would be necessary to pull the joint in two, but it was strained to such an extent that the value of these joints was very materially decreased—decreased to the extent to which Professor Kennedy has drawn attention, that would allow leakage; and had the strain been a little more, I think it more than possible we might have had some of the solid plates yielding in the adjoining streaks. My Lord, I think there is one thing to congratulate ourselves upon in these two Papers, and that is, they both close with a declaration that they are seeking more information. I think that is one of the most hopeful signs we have in connection with this subject. Nobody who has given it any thought professes to have reached finality on the subject. I think we are beginning to know how little we know, and I would join very cordially in the sentiment with which both Mr. Wildish and Mr. Milton have closed their Papers; and in the last words of Goëthe say, "Light, light; more light."

Mr. W. H. White: My Lord, I do not now rise to address the meeting, but to suggest that if Mr. Kirk would not mind saying a word to us on the "friction" question, he would contribute

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something worth listening to in addition to what has been given us in the Paper. I should like to say a few words before the discussion closes, but Mr. Kirk has given such attention to the question of friction in riveted joints, that I am certain he will do us a kindness by taking part in the discussion.

Mr. A. C. Kirk: Gentlemen, it is about the last thing I ever thought of, to come forward to speak. I do not know that I am acquainted with anything more about friction of rivets than other people. At all events, I have not got any data here. I think this proceeding of Mr. White's is perhaps a little irregular at this time, but since I have been called up, I will only state sentiments, because I cannot state facts. Now it strikes me that almost ever since we began to pay particular attention to the strength of riveted joints, we have unduly exalted the question of the ultimate strength of riveted joints, what we have been obliged at least to call the ultimate strength, because where the strength of a riveted joint really ends, whether it be in the ship breaking through the middle at sea, or in a steel boiler bursting, or at a stage previous to either of these catastrophes, is very difficult to say. Mr. West drew attention to a very important point, namely, reciprocating strains. They occur more in ships than they do in steel boilers, but steel boilers are not by any means free of them; not that the pressure of the steam brings reciprocating strains, but unequal expansion and contraction in heating and cooling does create reciprocating strains, and those have to be thought of. My feeling is altogether, as I have said, that we have exalted the question of the ultimate strength of riveted joints too much, that all the boiler makers of about the year 1870 and earlier were, in their instinctive fashion, not very far wrong when they over-riveted the joints, as no doubt they did in the way we look at it now. That is to say, they reduced the strength of the boiler, as it would be calculated now, by over riveting, but one result attained by that was, that the boilers were more easily maintained steam-tight, that there was more friction, and, in consequence, less working and slipping in the joints than we sometimes have now. Under the influence of the technical education that we have all been subjected to, part of it by the authority of the Government and part of it by the combined authority of the shipowners under our good and able friends here, we have gone into the very widest pitches that would satisfy the rules laid down, and in doing this we have consulted our own interest, we have consulted the interest of the shipowners, and we have contented the shipowners, because the contract is generally that the boilers shall conform to Lloyd's and the Board of Trade. When they have done that, they have done all that is wanted, the rest may follow. Now we have gone in, as I have said, for very wide pitches; we have gone in for thick butt straps, very often as thick as the plates themselves, simply because thick butt straps can stand caulking in wide pitches much better than thin ones, the wedging in of the iron in the caulking does not spring the plates apart between the rivets so readily. The friction of the plates is not so great as if it had more rivets in it, and the immobility of a riveted joint depends on the friction only. I am not aware—one has seldom indeed heard of it—that boilers (I speak of them more particularly) actually tear through riveted joints. At any rate, where you have double butt straps, as they are generally thicker than the plates, the question of corrosion, or any factor of safety to cover corrosion, in such joints, may be left out of account altogether and more rivets put in. I think I have now said all that Mr. White has asked for. I have not, in fact, here any data to give, although

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I did make some experiments upon the pressure with which the rivets grip the plates together in cooling, but I may just roughly say, that to the best of my memory, though it is a good many years ago now, the pressure with which a rivet in shrinking draws the plates together was—I am speaking roughly about its elastic limit—about half its breaking strength. I would add, that the pressure with which plates can be put together by a riveting machine is very considerably above that. Now, as regards the question of friction of rivets, it is of the utmost importance to know how the joint was riveted. If riveted by hand, to what extent was the hammering continued as the rivet cooled, because with a hammer a man brings a large amount of power ("horse power," if you like to call it so) into play upon the rivet. Each blow is spent through a very short space, and consequently the pressure is in such a case very intense; the rivet keeps what it gets, so that the pressure produced by each blow is continually added up by the rivet till the plates are held together by a very intense pressure. With a riveting machine, on an inch area rivet, you would probably put a pressure of 70 tons. Now if you start with the plates touching each other perfectly, our iron rivet, 1 inch in area, would, in cooling from a red heat, draw the plates together with about 10 tons, whereas our riveting machine, bringing 70 tons on the shank of the rivet, will crush up the shank of a cold rivet even, and press the plates together with a vastly greater pressure. Hence the propriety of keeping the pressure on the rivet till the shank is considerably cooled. This is the more necessary as thick plates have a great tendency to force themselves apart, and if the shank of the rivet is relieved of the pressure of the machine while still nearly red hot, the plates, in their endeavour to spring apart, will draw out the shanks and loosen the joint.

Mr. W. H. White: My Lord, I would apologise for the irregularity I have committed; but I am quite sure the Meeting will pardon me, because of the speech we have just heard. I ventured to do it because I saw the results of the experiments which Mr. Kirk made, and they seemed to me of the highest importance as showing that under ultimate strains—which is really common sense, if you think of it—friction is of little or no assistance to the rivets. Friction really is a help against strains that are much below the ultimate strains on a joint. These two papers appear to me to be among the most important we have had this session. Of course they belong to a special department of construction, but they have a distinct bearing upon what after all is a matter of the highest importance either in engineering or shipbuilding, that is, the getting of the utmost possible strength out of a given weight of material. When we have, either in ships or boilers, to combine separate plates and fasten them together unless the fastenings are adjusted with the utmost care, we must fall very much short of what might be achieved with proper precautions. Sir Edward Reed has alluded to the help I was able to give him—I am sorry to think how many years ago now—in summing-up what had been done up to the date of the appearance of his book in connection with experiments in riveting, I think we may fairly say that up to that time we did collate and summarise, and as far as we could, analyse all the materials that could be found. That was at least the endeavour made, and it was a very laborious task. There is no doubt that since that time much that is new and still more valuable has been done. A fresh collation is very badly wanted, and I hope that a member of this Institution—namely, Professor Kennedy, who has already done so much independent work, and who knows so much of what others have done, will be the member who will accept that invitation of Sir Edward Reed's, and will give us the benefit

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of his labours. As regards the experiments which Mr. Wildish records, it was my duty at the Admiralty, when the construction of the *Iris* and *Mercury* began, to look very closely into the experimental determination of the proper fastenings in steel ships, and a very great many experiments were made before any of the principal fastenings of the *Iris* and *Mercury* were arranged. We used iron rivets then because we could not get steel, we tried to get steel but we could not get proper steel, that is to say, steel that was trustworthy. We got steel which worked beautifully, but when we heated it by percussive strains, the rivets flew. Of course, steel rivets had been used years before in the inside work of many ships in the navy, but when it came to the question of the outside shell and the more important structural parts inside the *Iris* and *Mercury*, it was felt to be safer to use the iron rivets. I must say I was intensely surprised to find how much the iron rivets fell off in value in a steel plate, as compared with their strength in an iron plate, when we first made the experiments. Mr. Wildish gave the facts, and those facts were made the basis of the fastenings in the *Iris* and *Mercury*, and I believe they are perfectly satisfactory. They had certain disadvantages which Mr. Wildish has pointed out. Then we come to the making of steel rivets. The Landore Company certainly deserve credit for having given us first of all steel that we could trust. I am not going to advertise any particular company, but we do owe to them first of all the getting that steel. It can be procured now very widely, and as soon as ever steel rivets could be used safely in steel plates they were used. No doubt it is a very advantageous thing to do. Now steel rivets, as gentlemen present know as well as I do, are being used all over the country with perfect success; they are tested as all steel work is, but I think if iron plates and iron rivets were subjected to anything like such a testing as steel gets, there would be some very surprising results ascertained. That is one indirect advantage of the introduction of steel; we are proceeding on all sides in a more certain and more scientific manner. We owe a great deal to Lloyd's for the influence which they have brought to bear upon it, and now that I am out of the Admiralty I can say, without being thought a particular advocate of my old department, that we owe no less to the Admiralty who I think have given the profession the benefit of their experiments, both at the time when Sir Edward Reed was in office and since. There is one thing to which I do not fancy enough importance has been attached by Mr. Wildish; but having very nearly lost my life on one or two occasions in consequence, it is impressed on my mind, that is to say, the influence of the different ways of putting in rivets on behaviour of a joint. I am happy to know the experiments which we have heard of to-day have been made by a testing machine at Pembroke, which will, I think, bear comparison with any machine in the country. I can now say that until we had this machine at Pembroke, the one with which we did our work was simply disgraceful and dangerous. It was a simple lever machine, which Sir Edward Reed knows very well, and which had the habit of sometimes breaking the specimens in a very eccentric manner. I had my head very close to a sample once, there were only 11 tons of strain per square inch of section, and I felt safe, when the whole thing flew without any warning. This was a good steel plate which had been tested to 28 tons per square inch, and I began to think I was making a discovery in a way which was not at all pleasant. These were snap pointed rivets, and when I came to examine the sample afterwards—they were single straps—I found that the workmen, in snapping the points, had considerably damaged the material in the neighbourhood of the hole. This sample happened to be made by blacksmiths who are not very familiar with the work of riveting; I have seen many other

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cases where the mode of putting in the rivet, and perhaps doing local damage in the neighbourhood of the fastenings, has brought about very awkward results. It was my duty, having had to do with these experiments throughout, to draft the instructions for the treatment of mild steel on which the Admiralty have worked, and I believe are still working; and since I have been outside the Admiralty I have again looked very carefully and independently into the matter, and it is my belief that the practice of a counter-sunk point with the counter-sinking going right through the thickness of the plate, is for steel work, in all important parts of the structure, the most certain and trustworthy method of fastening. I should like also to say this—it really means very little additional expense. At Elswick at the present time, in the very light scantling war ships we are building, that system of counter-sinking is carried out by means of a stop on the drill at small expense, and gives us very excellent results. These are practical points. I thoroughly endorse what has been stated in recognition of the value of the two papers. With respect to Mr. Milton's paper, I think that Professor Kennedy has hit off the point of the greatest importance; that is, the question of the relative yield under the unequal strains of plate and rivet. Of course, Mr. Milton would be one of the first to say, let experimental determination settle that relation, not a mere mathematical enquiry.

Mr. H. H. Laird: My Lord, would you allow me, as my firm has had long experience in building steel vessels, and as I think we were among the first to use steel rivets, to say that our experience confirms what Mr. Wildish points out as to the advantage, or almost necessity, of using steel rivets in steel structures. There is one point about which he expresses a little doubt (on page 184) with reference to the partial restoration of strength lost by punching in the sample when riveted, by the action of the riveting. The opinion we formed, after a number of experiments, when considering the question of dispensing with annealing punched plates before using them was, that the mere fact of the hot rivet being put into the punched holes was sufficient to anneal that portion of the steel which had been disintegrated by punching. I think that this may be some explanation of what he mentions as being difficult to explain, and that the heat of the rivet itself really has an annealing effect upon the film of metal which is disintegrated by the punching.

Mr. J. G. Wildish: My Lord and Gentlemen, after the interesting discussion that has just taken place, I desire at once to thank you very much for the hearty reception my Paper has received. I have been especially pleased to listen to the criticisms that have been expressed, tending as they do to elucidate the facts, and to remove any misapprehensions that may possible have existed. It is satisfactory to find that Professor Kennedy's experience is so much in accord with the experiments I have had the honour of describing to you. With regard to the question as to the sectional area of the rivets, I would point out that the doubt raised, of whether the nominal or actual area was meant, is removed by reference to the third Appendix, which gives the exact area of the rivets when finished in the plates, and then the shearing stress per square inch of that area. The clause commencing at the foot of page 188 in the Paper, also shows that the area for the finished rivet is intended. With reference to the remarks of Professor Kennedy as to the diminution in the strength of the punched plates differing very considerably, I fully concur with what he has said in this respect. The plates that we tested were, as I pointed out, of only one thickness. In the experiments made by Mr. Kirkaldy, to which I have referred (in which

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the thickness of the plates ranged from quarter of an inch to one inch, and other circumstances affecting the strength per square inch of the plates at the section across the punched holes were not quite the same as the Admiralty experiments), very different results were obtained, and in the cases of the thinnest, or quarter inch plates, the material did not seem to be at all injured by the punching. The serious injury from this cause was found to occur in the plates three-quarters of an inch thick and upwards. I did not see the experiments I have described to you carried out, and cannot answer all the questions so well as I should like; but as to whether the holes were drilled* or punched in the experiments referred to on page 190, made to ascertain the frictional resistance of riveted joints, I will endeavour to find this out, and, if you will permit me, add the information hereafter. With reference to the remarks of Mr. Manuel, as to the holes being too near the edge in the test plates, shown by the diagrams B and C, it should be stated that they are only roughly drawn, the exact positions of the holes being given in the Paper. The breadth of the test plates represented by diagram B—that is, at the narrow part—was six diameters of the rivets. The rivets in those plates, and in the first eight of the tests shown by diagram C, were three diameters apart, so that we have on the outside of the two holes as much material as lies between them. The breadth is also given for the experiments represented by sketch C, and having a varying pitch of rivets of 3½, 4 and 4½ diameters. As to the material of the steel plates being improperly treated in attaching them to the testing machine in the way indicated on the sketches, I think the results of the experiments show that the material was not unfairly treated, so as to impair its tensile strength, by the method of attaching the plates to the machine. With regard to the question asked by Sir John Hay, as to whether the holes were punched violently or slowly, I would remark that the holes in the test plates used in the Pembroke experiments were punched by an ordinary punching machine, such as is commonly used in the ship yard. The operation was carried out with the usual slowness of motion in the punch, and certainly without any unnecessary violence. Then, as to the size of the punch and the die, or bolster—the second question asked by Sir John Hay—the punch used for the snap riveting, was about one-sixteenth of an inch larger than the nominal diameter of the rivets, in order to make the hole large enough to give the usual clearance, and the bolster would have been a little larger—about another one-sixteenth more all round—than the punch; but not having seen the experiments carried out, as I have just said, I am not able to give the exact clearance allowed in the bolster. Referring next to the question of machine *versus* hand-riveting, it is gratifying to find that the "machine riveter," which is, I think, a very useful instrument, has it advocates in this room. In my remarks on this question, it is clearly pointed out that when you are comparing machine riveting with snap pointed, or with conical pointed, hand-riveting, you have, as regards the friction, a great advantage on the side of the "machine" riveting. I have had the opportunity of seeing many thousands of rivets put in by means of the "machine riveter," and also of testing a large number of them after they have been put in, and I can vouch for the soundness of the work done. The rivets thoroughly fill the holes, and the plates are admirably well closed together. Occasionally you find the heads and points are not concentric with the body or shank of the rivet; but that is a difficulty which with a little care in the manipulation of the machine, can be avoided. The "machine riveter," in the form we now have it, can be readily used for such work as putting the frames of a ship together. For

* The holes in the plates were drilled, and the slots opposite the rivets in the middle plate were cut out by a machine.

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keel work it seems of special value, because the rivets are generally long and large: the machine squeezes or closes the whole of the work tightly together, and the rivets thoroughly fill the holes. The "machine riveter" is also useful for making up bulb plates and angles into deck beams. But I am not aware that a suitable machine has yet been devised for riveting up the shell or outside plating of a ship and the deck plating; and as this is so, it is, I think, reassuring to find—at all events, from the experiments I have placed before you this morning—that the results obtained with the countersunk "hand" riveting, as commonly adopted in the outside plating and decks, appear to be nearly as good as for the "machine" riveting, both as regards the ultimate strength of the rivet connections as well as their frictional strength. With reference to the observations made by Mr. Kirk, I concur with what he has said, and also with the remarks by Mr. White. This, my Lord, is all I have to say in reply to the questions raised in the discussion following my Paper.

Mr. J. T. Milton: Gentlemen, I must thank you for receiving my Paper so kindly and fairly, and a very few moments I think will suffice for all I have to say. Professor Kennedy drew attention to the small amount of difference of stretch in the parts of a joint in a treble riveted lap joint, and stated that it would be about 2 per cent. in the difference of the stress on the rivets, at a stress of about 5,000 lbs. on the plate.

Professor Kennedy: No; the difference between the plates would be 2 per cent. of the difference due to the squeezing of the rivets.

Mr. Milton: I, especially in this Paper, wish to point out that, comparing the results of riveted joints of different forms from ultimate strains, is an improper thing to do. When we come to ultimate strains, the plates stretch very considerably. When you get more than half the ultimate strain on the plate, the plate stretches very much. In a treble riveted joint we shall have one part of the plate stretch very much, and the other part stretch very little. If it was not for the fact that the rivets deform so very much in steel rivets, we should find a very great deal of difference in the strength of the rivets to what is shown. If we used rigid, hard rivets instead of soft rivets, the point to which I have drawn attention would be of a great deal more importance. Up to ordinary working strains, I do not think there is very much in the point, but we are using every-day results from ultimate strains to limit our working strains, and while that seems to be the practice of everybody, I think it only right to draw the attention of the Meeting to the point as I have done. Mr. West asked me about the quadruple riveted joints. I thought I was quite clear there, but what I meant to show was that in quadruple riveted joints, rivet area for rivet area and the other conditions being the same, there being two rows of rivets in the centre not having the full strains, at the ultimate strains the rivets must break at a less mean strain. In the case quoted, they broke at slightly less mean strain than in the other experiments. I say that that would have shown to greater disadvantage if the rows had not been closed up. Now, in these experiments of Messrs. Denny, breaking at about 20 tons per square inch, the rivet steel was carefully tested by Mr. Kirkcaldy. If those rivets had been tested singly instead of in combination as a riveted lap joint, they would have stood over 23 tons. As a matter of fact, they broke at about 20 tons, a difference of about 15 per cent. Mr. Kirk has made some remarks, very straight to the point, as to the pitches of rivets in boiler shells. Undoubtedly, the

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tendency lately has been to make them a great deal too wide. Boilers are at present being made—they are not at work yet—with $\frac{11}{16}$ plates, and the pitches of the rivets are about $9\frac{1}{8}$.

Mr. Kirk: $9\frac{1}{2}$ inches.

Mr. Milton: $9\frac{1}{8}$, and the plates are fitted with double butt straps, each butt strap $\frac{13}{16}$ thick, each one thicker than the plate, for the purpose of getting the caulking edge.

Mr. Kirk: That is an enormous distance.

Mr. Milton: It is a very great distance, but those boilers have been tested. More than usual care was exercised in fitting the butt straps, and they were perfectly satisfactory under the hydraulic test. What the shipowner will think of them in a few years' time, is a matter for his future consideration.

The President: Gentlemen, I am sure you will allow me to convey to Mr. Wildish and to Mr. Milton our united thanks for these very valuable Papers. As the question of irregularity has incidentally arisen in the course of this discussion, I know you will permit me to make just one remark, because I am responsible for our regular proceedings, being in the chair. When one eminent man has so high an opinion of the views of another eminent man, I think it is the duty of the Chair not to deprive the Institution of the advantage of hearing his friend, because the public would be losers, and you would yourselves be losers also. Therefore I conceive it to be the duty of the Chair to overlook any slight irregularity that might occur if we made a practice of allowing one member, so to speak, to poke up another.

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ON THE MANUFACTURE OF LARGE FORGINGS FOR STERN FRAMES.

By D. Purves, Esq., Assistant to the Chief Engineer Surveyor to Lloyd's Register.

[Read at the Twenty-sixth Session of the Institution of Naval Architects, March 27th, 1885; the Right Hon. the Earl of Ravensworth in the Chair.]

The importance of having sound and reliable forgings for stern frames cannot be too strongly impressed upon those responsible for the construction of our merchant steamers, but it has apparently been overlooked in many quarters.

I venture to bring this subject before the members of this Institution, in the hope that that attention will be given to it which its importance deserves.

It is true that lately a few enterprising individuals have striven earnestly to improve upon their previous practice in making such forgings, and have put down steam hammers and other plant in order to enable them to turn out a more trustworthy job; and, in fact, there can be no doubt that it is now possible to obtain a better forged stern frame than it ever was before; but, at the same time, frames are being made, or rather I should say attempted to be made, which are altogether unfit to be placed in vessels. In this Paper I propose to notice the methods of welding together the pieces which go to make up a frame, and also the positions in which the welds are placed, and shall endeavour to point out what, in my opinion, are the advantages and disadvantages of the various methods; but I shall abstain from mentioning where these practices are in vogue, as shipbuilders and others interested may easily ascertain which practice is in use in the establishment where they obtain their forgings, and can draw their own conclusions.

In the first place, I would observe that as these frames are too large and cumbrous to be forged in one piece they are usually forged in separate pieces which are afterwards welded together, and as the welds are necessarily weaker than the solid forging, it is well at the outset to have as few of them as possible, and to have those which are inevitable in the places where they are least likely to be strained.

In practice it is found necessary to make the frames in at least four pieces, and

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it is also found that four pieces are quite sufficient for even the largest frame, so that only three or four welds are necessary in any frame.

In speaking thus of the welds, I mean of course those which unite large pieces together and not the continuous welding or building up from slabs into what is usually called a solid forging. One of the most common ways of making these, till quite recently, was the V screwed or butt weld, in which the two pieces to be united are fashioned of an obtuse V, very often little better than a straight butt; they are then placed nearly in contact in the fire, and when they are raised to a welding heat are drawn together by chains and screws. Such a weld with large masses cannot be perfect, as it is impossible to obtain sufficient power to draw the two parts together, for there can be no comparison between the closing effect of the static pressure exerted by the screws and that due to the impact of the heavy blows of a steam hammer, and another fault in this kind of weld, as usually made, is that the V is not deep enough. Another kind of weld is the V weld hammered instead of being merely screwed. If V welds of any description are used, the length of the V on each face should not be less than the width of the piece to be welded, and the parts should be very carefully shaped before welding is attempted, so that they will, when brought together, be first close at the root of the V; the cinder has no chance of being shut into the mass of the iron as it has if the parts are so fitted as to be just touching all along, or if they are closed at the outside.

If the angle of the V is made more obtuse than I have indicated, the parts are not well closed together by the hammering, while if it is very obtuse the more they are hammered the less tendency there is to close them together.

Another form of weld is the double-glut weld. In this form each of the pieces to be united is shaped an obtuse V, so that when they are brought together they leave two openings, one on each side, which are filled with the gluts. One of these is usually, but not invariably, made considerably larger than the other. The ends are raised to a welding heat at the same time as is the glut piece, and when the first glut is put in place it is hammered by a two or three shafted hammer; the frame is then reversed and the other glut put in in a like manner.

With this form of weld it is impracticable to use a heavy steam hammer, for when hammering is applied in such a case it is more a hammering tending to the separation than one tending to the mutual incorporation of the surfaces. When the wedge piece has become connected at a portion of its surface it is apt to be broken off from the first

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junction and it may not be again brought into welded union with the butts. The surfaces of the weld ought further to be so disposed that the direction of impact of the hammering has a considerable component normal to the surfaces to be brought into union.

There is also an objection to this multiplication of surfaces without a greatly increased area of effective section. In a transverse-glut weld each welded surface is less than the section of the post, and there are two pairs of surfaces to be made good, there is therefore more probability of having a defective junction, for unless both sets of surfaces are good neither is of value. A further form of weld practised in a few establishments for one of the welds of the frame, is a scarph weld. This, if properly made and well hammered, cannot be improved upon; and, in my opinion, the next best form of weld is the long V weld properly closed together under a steam hammer.

To show the relative value of welds made by screws, hand hammering and steam hammering, I have here brought some large specimens of welds made on the V principle expressly for the purpose of testing, so that they are at least as good as any welds can be made in the same way in ordinary work.

The piece marked No. 1 was scarphed and screwed together, heated and welded with ordinary sledge hammers. The two pieces marked No. 2 were also scarphed and screwed together, and they were hammered with a large three-shafted hammer weighing about two hundredweights.

The pieces marked No. 3 were scarphed and put together, and at a welding heat hammered under a steam hammer that had been specially made for the purpose.

These pieces were afterwards made red hot and bent across the welds. The piece marked No. 1 bent about 30 degrees, and in endeavouring to bend it back the weld gave way; the pieces marked No. 2 were bent in a like manner, and also broke without bending back; the pieces marked No. 3 were bent to an angle of 30 degrees at a red heat, and straightened several times without showing any signs of giving way, and it was only after the piece became black hot that it could be broken by repeated bendings and then through the solid.

It will be seen from the scarphs of these pieces that those made under the steam hammer are far superior to the others.

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I have already stated that four, and only four, welds are requisite in stern frames, and now I will discuss their best position.

In large frames two welds are made in the upper arch of the frame, as shewn on the diagram on the walls. These welds are often made too close to the large mass of metal in the posts; this mass must exercise a prejudicial effect on the temperature of one of the parts to be united, and it is good practice to make them as far from the posts as possible.

The same thing applies to the weld uniting the forward post to the keel-piece.

The remaining weld is often made in the keel-piece itself. This is, in my opinion, a most improper place for it; it should always be made in the after-post, above the heel of the rudder. Many a steamer has lost its rudder-post through the failure of the weld in the keel-piece; when the foot goes, the post soon breaks off at the top of the screw opening, and the rudder goes with it. There is no reason whatever why keel-pieces should be welded in that place, and if it is but thoughtfully looked at for a moment, I believe no shipbuilder or shipowner would ever allow such a construction to be adopted where his interests are at stake. When the weld is in the keel-piece at the foot of the gap, the bending moment acting upon it is of course the pressure of the rudder with a leverage, which is the length from the pintles to the centre of the weld, say from two to three feet. On the other plan, when the weld is above the rudder-heel, the pressure is much less, and it acts now with only a few inches of leverage, and therefore properly made welds never give way there. The likelihood of failures is therefore greatly reduced by this better disposition of the weld; but there is another important advantage in having it at this place, it is above the rudder-heel, and even if the weld does give way, the rudder is still supported at the heel, and retained in its place.

These views on the subject of welding stern frames, are the result of a long experience and much thought given to the subject, and I am convinced that if all frames had been made in the manner I have indicated, we should have had far less experience of broken stern frames than we unfortunately have had.

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DISCUSSION.

Mr. B. Martell: My Lord, although this is a very short Paper, and one of a perfectly practical character, it is one of very great importance. We have found that in crossing the Atlantic, large ships are subjected to heavy seas and gales and in many instances break their stern frames, owing to imperfect welding and to the weld (as Mr. Purves has pointed out) being improperly placed; these vessels have had to be put into dry dock to be repaired at New York and other ports in America, where the expenses are very great. The consequence has been that underwriters have been subjected to enormous expense on account of those accidents, which I may call to a great extent "avoidable" accidents. Such accidents were found to be so numerous that the Committee of Lloyd's Register determined to appoint inspectors, men practically acquainted with large forgings, as independent surveyors to the Society, and to station them at various ports throughout the country, so that they could visit the establishments where these forgings were made, inspect them, and see for themselves that the inefficient manner in which hitherto these frames had been made should be discontinued. I may say that Mr. Purves, who has read this Paper—because it sometimes is necessary that one should know a little of the man who comes before us, in order to place that amount of confidence in his opinion which is necessary to enable it to carry weight—is a gentleman of very extensive and practical experience on this subject. I may say he is not only a most excellent engineer, but in addition to that, he has had charge of large forging works where an enormous amount of work of this kind has been conducted under his superintendence. The Committee some time ago thought, that in view of the various practices of making these forgings in England and Scotland, it would be well for Mr. Purves to go round and consult their resident Inspectors of Forgings at the various ports, and endeavour to influence the makers for good in pointing out the best practices, and the most efficient ways, of welding stern frames and other large forgings. He has been round, and he has made a most intelligent and interesting Report to the Committee of Lloyd's on the subject, and this Paper is the outcome of that visit. I have no hesitation in saying, from my experience, that if these plans that are brought before us now by Mr. Purves are thoroughly considered by the manufacturers, and the plan which he has pointed out as the best be adopted, there would be an enormous saving to the country with reference to accidents which as I have stated are perfectly avoidable. It is not only the expense of re-welding the stern frame, I may say, that is incurred, for one can easily understand that when a stern frame is broken in this manner it frequently carries away the rudder with it; in those cases, if in a heavy gale and a ship gets into the trough of the sea, she is very likely to be lost, and I dare say many steamers have been lost on account of this. Only very recently (indeed I scarcely know whether the ship has left London yet) I went down to the docks, and found that one of these ships coming across the Atlantic had leaked considerably, a lot of water had found its way inside, and on examining her we found that some of the rivets had got loose at the after part. On further examination we found the stern frame was broken just about the part where *that* line is seen in the keel of the vessel (*pointing to the diagram*). The whole of the frame had to be taken out, and it had to be taken to the Thames Iron Works and re-welded, and put back again, involving very large expense indeed. As Mr. Purves has pointed out, there are places where it is perfectly absurd that welding should take place, where it is likely to prove the weakest part because it cannot, under any circumstances, get the

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same homogeneousness as the material itself, and by the exercise of a little intelligence, such as has been displayed here, many of these accidents might be avoided. I think, Gentlemen, this is a valuable Paper which will do credit to this Institution, as showing that it is not only questions of scientific interest which are noticed in this Institution, but that matters of real practical importance, affecting in a pecuniary point of view shipowners, underwriters and merchants, are dealt with by papers of this kind, and that those papers are equally well received here, and their importance thoroughly appreciated.

Mr. McLean: My Lord and Gentlemen, I have listened to Mr. Purves's Paper with very great pleasure, and with the more interest, as it is so valuable from being the result of long knowledge and experience. I do not know that I can say much about it further, than in corroboration of what he has advanced. I may say, however, that having been engaged in the making of those heavy forgings for the last thirty years, I have had a good deal of experience, and I may, perhaps, be allowed to add, to those who know the Lancefield Forge, not without a fair share of success.

I may remark with regard to the *positions* of the weldings, I perfectly agree with Mr. Purves that no one would choose to make the keel portion of the frame to shut in the position indicated, if he was desirous of making a very sound and satisfactory job. Frames ought to be shut where it is indicated, at the back post and inner post; again, as regards the one at the top, as he mentioned, not in his Paper, but as it is indicated in the drawing, they are generally shut with one instead of two weldings. Indeed, it is only in the case of a very large frame, say of ten feet width or so, where the projections of the two posts cannot properly be made to meet together without the intervention of a middle piece, that the two weldings are necessary, and the intermediate piece thus put in to the bow or arch of the frame necessitates an additional welding.

With regard to the *nature* of the weldings, I am quite of his opinion as to the insufficiency of the butt weld, also of the V weld; and I cannot conceive of any one shutting a frame with a V weld who could possibly avoid it, and for this reason, that it is usually heated in a smith's fire, and the pieces being placed together to be screwed up they are only brought sufficiently near to allow the flame to play between the surfaces, and thus you can never get these surfaces clean, it is almost impossible, because although the cinder itself may run out, the gases from the fuel have a tendency to form a skin or scum upon the two surfaces sufficient to prevent the proper cohesion of the metal when screwed together. I have seen many forgings in this condition. I saw a frame, not above a fortnight ago, where the V part was as black and smooth as your hat, showing no cohesion except at the edges. I am not sure but that many similar cases may be in the experience of many who hear me.

With regard to the scarph-weld, there is no doubt it is the best weld where it can be applied. It is always adopted where you can get the pieces into separate furnaces, because you have the clean surfaces to work upon, and you can get the steam hammer properly applied. The glut-weld, as Mr. Purves calls it, is the weld that is usually made by us, and I do not know a better. It has been in use with us for the last thirty years, and in all that time I may say that I am not aware, except in one case, of any instance in which a defective weld has been found. I can point to the majority of the Cunard Company's steamers, such as the *Servia*, the *Aurania*, the *Catalonia*, the *Pavonia*, the *Oregon*, the *Umbria*, &c., and very many of those of the other large Companies, such as the Peninsular and Oriental, the Royal Mail, the

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Union, &c., &c., and I can say, that, as to these frames, and other nearly as large ones, we have never had a complaint with reference to any. The object of the glut-weld is this, and probably it will be quite apparent to all those who have anything to do with such work. In putting together the pieces of the frame, the back post, the inner post, and the keel part, they have to be kept (by means of glands or binders) firmly in position, even to an eighth of an inch; a shipbuilder will not take the frame from you without it is quite to his mould, and we are consequently obliged to have it exactly to his dimensions. This precludes the possibility of the scarph-weld, the pieces being fixed so close in position, and the glut-weld has to be adopted. This one, as shown, is rather exaggerated, it should not be so deep or so acute (referring to diagram on wall), the V of which apparently comes out on the other side, and is hammered there. However, the object of the glut-weld is, that when you have heated the upper surfaces, on swinging your frame from the hearth to the anvil you can clean the surfaces thoroughly by means of heather brooms, which we use continually before applying the filling-in piece, which is brought to a welding heat in a separate fire. The consequence is we have a perfect weld, the two surfaces cohere under the action of the hammer; but to make a perfect shut you have to turn the frame over and heat and weld, in the same manner, the other side. In the case of a heavy frame, this is a very difficult matter —with our arrangements we can turn over a frame having a length of keel portion of about thirty feet— of course it nearly touches the roof, but we get it over.

The great secret of making those welds perfect lies in the proper heating in the hearth, which we obtain by abandoning the usual method of heaping up small coals over the part to be welded, forming what is called a hollow fire—this we deprecate, and prefer instead, heating with coke, and cleanly confining the flame on the surfaces by means of a fire-clay cover hung from the roof, and actuated by a lever, imitating, in a small way, the clear forging furnace, and having the proper appliances to move the frame quickly from its surroundings to the anvil so as to enable you to "strike while the iron is hot," and to enable you to turn it over in order to make a perfect weld on the other side.

I think, Gentlemen, you will find that except such means are adopted to have the surfaces perfectly clean you need never expect to make those perfect welds which are so indispensable in such important forgings.

Mr. A. E. Seaton: My Lord, as we have four patent slips at Hull, on which we do a large amount of repairing work, it necessarily follows that we see a considerable number of the defects spoken of already; but there is another point to which I should like to call attention. It is, perhaps, somewhat foreign to the subject, but at the same time it is interesting. We have observed on all occasions that the straining immediately abaft the main post has been very considerable; and that when a ship has caught her heel on the ground, or in dragging over a mud or sand bank, apparently very little force has broken the sole, immediately abaft the body post. In all these posts there has been no weld there; the weld has been always, as I believe is usual, in the main post, just above the keel. We have found it preferable, instead of attempting to weld the post, or to go to the expense of putting a new sole piece in, to fish the post, by means of very heavy steel angles on each side and a flat plate on the bottom. There is no doubt that what has been said is perfectly true, that in whatever way the welds are made, two or three out of ten will probably be faulty, and when a strain comes upon them, they give way. But the shape of the post is a point to which I think I may now call attention, especially as what I may call the evil of the post is

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immensely magnified in the sketch which we have before us. The bending moment caused by the rudder on the sole piece is, of course, greatest at the junction with the main body post. You see before you a drawing in which the vertical thickness is the same throughout. I do not think even that is right, because the post has to carry the *weight* of the rudder besides having to bear the strain of the rudder in steering; now, in fishing these posts, we always corrected what we deemed to be the grave error in their form; that is, we gave a greater breadth in the sole immediately below the body post, at the point where the greatest bending moment comes. Posts fished in that way have proved themselves, by severe trial and long service, to be superior to the original condition. As we could not get a post forged conveniently, or except at considerable expense, of such a form as to carry out our ideas, we resorted some years ago to the use of another material. At that time my friend, Mr. Hall, of Messrs. W. Jessop's and Sons, apparently had the same thing floating in his mind, and when he broached the idea to us of a cast steel post, I was, I believe, the first of his friends who took it up and helped him to carry it through. We have since then fitted very many steel posts, and have thereby avoided, what I consider the doubtful point, viz., welds, and carried out our views as to the best form for strength of this kind. Very recently we fitted to a ship a steel post, weighing from 14 to 15 tons. It was considered very naturally by the owners, and by ourselves as well, that the making a steel post in one piece would be a serious matter; as, in the case of breakage to that post it could not be welded, and if it could not be fished it would mean an entirely new one. It also occurred to Messrs. Jessop, at the time, that they could not send us this particular post by rail; the consequence was, that what at first appeared to Messrs. Jessop to be almost the cancelling of the order, really helped us to get over our difficulties, for it caused us to design it in two pieces, fastened with a long scarph joint below the boss, and another scarph in the after post, below the bow. The post in each case was very much increased in fore and aft breadth in way of the scarph, in order that there might be through the riveting exactly the same strength that there was through the body of the post. We submitted the question to Lloyd's, and, as far as my memory serves me, Mr. Martell made no material alteration in it. On the whole, the design worked out very well indeed. Now there is still another point to which I should like to call attention, if I am not trespassing on the time of the Institution. That particular stern post enabled me to carry out something which I had long had in view; that was, the better connection of the stern frame with the main structure of the ship itself. I think any one who has seen in a large ocean-going steamer, what after all is a very paltry connection between the huge forging and the skin and stringers and framework generally of the ship, cannot have considered it satisfactory, nor felt that it was right that such an important part of the ship as the stern post should be tied in such an inefficient way. We, therefore, took advantage of the fact that we could, without any extra expense, get it cast to any form we wanted, and took the opportunity of having certain flanges and ribs, and by which we connected the posts of this ship in a way that had never been done before. We were able to modify the usual transome frames so as to very materially strengthen the stern of the ship itself. We carried out in that and several previous stern frames what we deemed to be the essential thing; that is, the material strengthening of the post by the change of form rather than by the amount of material, to resist the great bending moment which comes upon the shoe-piece at the body-post. There is one thing that is not shown on the sketch, but is no uncommon thing, especially in German ships, viz., the outer bearing for the propeller in the after post. I maintain that that is radically wrong, for after careful examinations made during the past twelve years, I have come to the conclusion that the

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post, instead of helping to carry the weight of the shaft and propeller, and steady it, has really been supported and steadied by the propeller shaft, and, therefore, many years ago we decided to do away with the outer bearing, as being utterly useless from an engineering point of view. No doubt it was a good thing from a shipbuilder's point of view, because many a weak stern-post has been saved from destruction or fracture by the support which it has thereby got. I think each tub should stand upon its own bottom; that the rudder should be dependent on the stern frame for support, and not on the stern shaft. In our own works there is a difference of opinion between our foremen as to the best method of welding. In the engineering department, the smith, a very experienced man, insists upon the scarph-weld; and I have never seen him make a single failure. His method is to form a temporary weld at the middle, with the two extreme ends of the scarph turned somewhat up, so as to leave free access to the fire, and after that has been done, to treat each end of the scarph in turn, and finally put the whole thing under a steam hammer. I have seen many connecting and piston rods mended in that way. I do not say very large ones, because I should not like to run the risk of a weld with such, but I have seen rods up to six or seven inches mended in that way, and after turning there was scarcely a trace of the weld. In our ship department we usually weld iron stern frames with the long V, as shown. They are, generally speaking, successful, but I was very much horrified a short time ago to see the arm of a large stern bracket for a twin-screw ship drop off with the shock of the steam hammer, although judging by the eye on the outer part it seemed perfectly welded, not the least sign of a scarph; where it broke off, it showed the two V's had fitted perfectly, but between them was a thin film of quite transparent slag which prevented the metallic contact. Our shipsmiths are very prejudiced in favour of the V-weld; they profess they are able to get much better jobs than with the glut-weld. Personally, I am in favour of the glut-weld, for the reason that Mr. McLean has urged, that when you take it out of the fire you can sweep it perfectly clean before inserting the glut.

Mr. J. F. Hall, of William Jessop & Sons, Limited, Sheffield: My Lord and Gentlemen, I must say that when I first read this Paper I was not surprised—yet still I was surprised. I was not surprised that a gentleman with the intelligence and foresight of Mr. Purves, belonging to an Institution like that of Lloyd's Registry, and daily having to look thoroughly into these matters, should think it worth while to read a Paper upon this subject; but I was surprised that the necessity still existed for reading such a Paper at all. With regard to welds, the matter has long been settled in my own mind: do without them altogether, there is no necessity for them; they are simply a relict of the past, and, I think, the last relict, as far as iron is concerned. Iron has had to give way to steel in boiler plates, in ship plates, in rivets and in ordinary forgings, and it will have to give way in the matter of stern posts and rudders. Of course, I have had a good deal to do with the manufacture of stern posts and rudders in steel castings, having, as far as I am aware, originated the idea, and the success that has resulted from those that have already been made under my superintendence has been such as to have been very grateful to me. I had the honour last year of reading a Paper before this Institution, on cast steel for crank shafts. That Paper met with a great deal of criticism as you know, both here and elsewhere. One of my principal opponents asked me, why it was that I had chosen that subject in preference to the one of stern frames and rudders, with which I was having such success. My answer to him was, that the stern frame and rudder question was so simple, and every one to whom I

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had spoken, who had taken any interest in it, was so satisfied it was right, that I did not think it worth while. The other question was so much more difficult, and being somewhat of a pugnacious nature, in the same way as my friend, Mr. Macfarlane Gray, said yesterday, being the more difficult subject of the two, I preferred to tackle that sooner than the other, which was simpler. With reference to the stern frame which Mr. Seaton has spoken of in the *Martello*, there is no doubt it was a very good job with regard to the way in which it was fitted up. I think that iron manufacturers, as I suppose they will still continue to exist for a short time, and will have some stern frames to make, might learn something even from that stern frame. I will just show you as exactly as I can on the black board how that stern frame was fitted up. There was a large scarph two-thirds of the way up the post of stern frame nearest the rudder, that partly extended outwards, and as it were robbed a small portion of the rudder, which of course did not matter. It also came a little inwards on the propeller side, and was neatly rounded off for appearance sake. The total area of the riveting was thus made more than equal to the strength of the sectional area of stern post itself. The other scarph was one-third up the inner post of stern frame, but came very much further inwards to attain the same object of rivet strength, and, of course, as you will readily perceive, it did not interfere in the least with the construction of the ship, I think that even wrought iron frames made in this way would be far superior to any welded ones. You are all perfectly well aware of the accidents that have occurred at different times through welding. The best workman in the world cannot always judge whether he has got a perfect weld or not, because the faults are hid. In the method we now employ in manufacturing the frames, if there are any defects you see them on the surface; they are always outside and are not hidden like these welds are. There is not anything said in this Paper about the rudder, but I suppose the rudder belongs to the stern post, and, therefore, I may be allowed to say a word upon that subject. The way we are making rudders now is to make them solid, all of one piece in a steel casting, without a rivet hole in it at all, so that the rudder is all giving strength, one part to another. We all know that during the present month, three of the best known steamers that sail out of Liverpool to New York have lost their rudders. I should like to know in how many of them there were bad welds. In one of them I know there was a bad weld. These things are constantly occurring. I suppose in the history of shipbuilding more disasters have occurred through bad welds than from any other cause. You have it in almost every part of the ship, or rather, you had it in olden times; you had it in the plates, in the boilers, in the rivets, in the propeller shafts, in the cranks, in the different parts of the engines, in the stern frames and in the rudders; but, as I have said, the matter has been got over now, and there is no necessity for any welding at all.

Mr. W. Putnam: My Lord, as an iron manufacturer, I am quite prepared to tell the last speaker that we do not consider the iron trade yet quite extinct. He mentioned that steel always showed its defects upon the surface. I have had a great deal to do with large masses of steel, and that is quite contrary to my experience. He points out that in his method of making the frames a scarph will do and will do very well. "What is sauce for the goose is sauce for the gander." If you can do without welds in a steel frame, why insist upon them in an iron one? My contention, as an iron manufacturer, and a maker of these large forgings, is that they can be welded with tolerable accuracy and certainty. There is an illustration staring you in the face (*pointing to No. 3 specimen*) that has been done by proper methods.

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I would like to ask the Author of the Paper how it happened that their Register permitted the state of things which existed for so long a time to go on as it did? The *modus operandi* has been something like this. The shipbuilder in keen times wants to buy in the cheapest market. Forge makers call upon him, leave a card, and quote a price for either a big frame or a little one, no matter whether properly laid out for making them or not, frequently the lowest offer is accepted without any knowledge on the part of the buyer whether the firm is capable of doing the work or not. You have the result there (*pointing to the test welds*). Certainly, "Lloyd's" are looking better after this now, but I contend that many of the failures they are complaining of so seriously to-day, and not only here, but at other meetings I have attended, are more their fault than the fault of the shipbuilder or manufacturer. Mr. Purves mentioned two welds there (*pointing to the arch in the diagram*). Two welds there are not necessary in all well-ordered forges. It is the practice to have only one weld in the arch. For my part, I do not see the need of a weld there (*pointing to the weld shown upon the bottom of the after post on diagram*). Except in very large cases, the forging can be made all in one piece, and bent at the bottom, you then have a curve at the bottom of the after post instead of a right angle, and in case of the vessel going upon a mud bank or anything of the kind, you lessen the tendency to break as described by Mr. Seaton. With regard to the welds the double glut-weld is exploded. The V-weld is the one I have the greatest weakness for; but I may point out it is not fairly shown. Reference has been made to its closing in the cinder. That is not necessary if the scarph is properly made. The two scarphs should only touch at three points, leaving the other parts sufficiently open to allow the cinder to escape. With reference to Mr. McLean's remarks as to the frames having to be made to the eighth of an inch, that applies to all frames, no matter what the scarphs are like. Our practice is to block the frame together and fasten it in all directions so that it cannot get away. The scarphs have a thick projection, when it has arrived at a welding heat it is brought under a powerful steam hammer, and the hammer has plenty of surface to play upon. The superfluous material is forced out at the edges and cut off. Of course, you cannot guarantee the work of every smith. A man, however careful he may be, may make mistakes. Neither can you guarantee, if you cast them in steel, they shall be free from blow holes. The question of whether they shall be of iron or steel will have to be solved in the future. Of course, we know what iron frames are because time has told us what they can do, what their failures have been, and their best properties. Steel has not yet had time. I make steel forgings as well as iron forgings, but I am not yet prepared to say I am convinced steel for all purposes is the best material. I would like to ask those gentlemen who are so strongly advocating steel for everything against iron, and who pooh-pooh iron manufacturers, and say that they must not give an opinion, why they should be satisfied in their own minds that *steel only is the proper* thing to use? It strikes me, as far as my observation goes, that the railway companies have had the greatest experience in the use of steel forgings for crank shafts, simply because they were the first who went extensively into the practice. Now we have acquired the means of knowing the result of that experience. Every railway company is bound to furnish the Board of Trade with a return of their broken axles. I have had the privilege of examining the returns for the last ten years; and the returns of those broken axles are considerably in favour of iron over steel, and that has been continuous from the date at which the Board of Trade adopted the plan of having these returns.

The President: Mr. Putnam, of course I am in the hands of the Meeting; but will you allow me to say we are not now discussing the question of railway axles, we are discussing steel forgings

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for steamships. I think it is necessary to keep to the subject matter, or else we may drift away into I know not what.

Mr. Putnam: Of course, my Lord, I bow in assent to what you have said. I was rather drawn on to go into that seeing that the subject of steel *versus* iron was introduced; but if that is the feeling of the Meeting, I will say nothing further upon that matter.

Mr. G. Duncan: In the midst of the scientific gentlemen who are assembled here, it will appear rather bold on my part to say anything. I am a sailor, and I was a shipbuilder. I learnt to be a shipbuilder before I became a sailor, and therefore I know something of the mechanical work connected with ships. But what I rise to say, my Lord, is something to indicate the feeling of underwriters in reference to this important subject, and it goes further than just the pecuniary loss which arises from the accidents that occur; because, as has fallen from some of the speakers, I believe a good number of ships are lost and never heard of again, in consequence of the accidents which have happened in connection with stern posts. An accident happens, the rudder is destroyed, the vessel is left lying in the trough of the sea, and down she goes.

Now, my Lord, as to the underwriters. When we get a very big ship in a foreign port, with a broken stern post, shipowners are sometimes very apt to claim for a constructive total loss, that is to say, that to manufacture another stern post in place of the broken one, would cost as much as the ship was worth, after being repaired. That is not always the case, but it is always the most expensive thing to do. So much so, that all wise shipowners make a contract for the doing of it with some shipbuilder; because, when they have to take out a forging, or even a casting, if it was a steel one, it is 14 or 15 tons; and on purpose to get it out, you have to cast adrift all the riveting in the ship's stern together, before it can be got away, and very generally this fourteen or fifteen ton piece is landed in the bottom of a dock; it has to be hoisted out of the dock, and put away where it can be dealt with by the smiths and their fires; so much so, that on one occasion I saw a weld going on in the Birkenhead Dock in a large vessel (one of the White Star ships) that had broken her stern post, and it was being welded in the dock. They positively were doing it there, and they actually brought a blast furnace and put up a fire there. The operation of taking out and mending the stern post was so serious, that rather than attempt to do it, they set to work to have it welded at the bottom of the dock; and they did it; whether it lasted I am not in a position to state. There is no question but that this is a very important subject indeed. We have not many things connected with the construction of vessels which are of much greater importance than the manufacture of the stern posts. The greater part of a ship is composed of small pieces, and can be put together safely and securely in the way indicated here in the forenoon, by making butt straps and other things connecting the two pieces together, but this is a thing that has to be done by welding or by piecing over. That is one of the things that has frequently to be done, as has been indicated here by the gentleman who spoke before me; in place of attempting to take the stern post out, they scarph pieces over both sides, and on the bottom of it, so as to endeavour to make it as strong and place it in the same position as it had been in before. Therefore, that this matter should be discussed by scientific investigators, like those who have discussed it here, and get the experience of the gentlemen who have been handling these things constantly, to see how best it can be done, must be a matter of great importance to the Mercantile Marine. There can be no question about it. I happen to

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be a Member of Lloyd's Committee, and therefore I am a little upon my defence in that direction. One of the gentlemen has charged us with being blameable for these broken stern posts, and so forth; but that same gentleman very possibly could tell you, that when we interfered with some other things, they thought we were going further than we had any right to do. A long time elapsed before the engineers would allow us to interfere with the engines at all; they said, you have no right to meddle with the engines, and you must not interfere. Now, my Lord, we were doing our best—I speak for Lloyd's Committee—when we found that things of that sort were done, large and important weldings of this kind were done in places where they had not the proper appliances, and by parties who had not the proper experience, and many stern posts were coming out, we felt it our duty to take the superintendence, and to go to these parties, and indicate to them how, by the experience of others working in the same direction, the work they were engaged in could be done in the best way. Gentlemen, I believe that has been of great service to many of those parties who were doing bad work before, and that they have to thank us for showing them how they can do it better.

Mr. D. Purves: My Lord and Gentlemen, I am very much obliged to the Meeting for the way in which this Paper has been received. I think it will be sufficient if I answer the questions generally. With regard to the advantages of one form of shut over another, I may say that butt welds were adopted generally up to very recently on the north-east coast, and the result was that they sometimes broke while they were being fitted into the vessels. In this district this description of weld has been abandoned, and the long V-weld substituted in its place. Still it requires a great deal of pressure to induce forge proprietors to use the steam hammer, which is necessary to make a thorough good job of it. We find that by using the screw alone the welds are very unsatisfactory.

With regard to the double-glut weld, I do not think that is a good plan either, because it must be remembered that in welding a large frame the glut pieces have to be carried from the smith's fire to the frame by the men, who have to stand close to the heats and drive the glutts into their places with a double-shafted hammer. It seems to me rather too much to expect the men to make a good weld in the case of large frames under these circumstances. But as I have already pointed out, although hand hammers will not produce a satisfactory result, this form of weld does not admit of the application of steam hammers. The long scarph is a good form of weld, but in the case of large frames it becomes too long for the smith to manage. You get the same result with a V-weld one-half the length.

I have seen a great many of these forgings give way in consequence of the weld being made in the keel part of the frame. Many years ago, in Liverpool, I saw two of the large Atlantic steamers in the docks at Birkenhead, at the same time, without their outer posts and rudders, having been towed in, through the welds giving way in this position. I believe there are as many welds made there as in the outer posts. I do not know why it is done, but it is still almost the universal practice in one very important district yet. Mr. Seaton mentioned that some of his smiths made welds so that they should have metallic contact. I have never seen that result obtained in practice. I have never seen a weld made but what there has always been a thin film of silicate of iron left between the surfaces. My Lord, I do not think I have anything more to add to what I have already said.

The President: Gentlemen, allow me to thank, as I know you will, on your behalf, Mr. Purves for his excellent Paper.

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ON THE EFFICIENCY OF MARINE BOILERS.

By J. T. Milton, Esq., Member.

[Read at the Twenty-sixth Session of the Institution of Naval Architects, March 27th, 1885; the Right Hon. the Earl of Ravensworth, President, in the Chair.]

The struggle for economy in the production of power for the propulsion of vessels, which has shown itself during the last few years by the gradual increase of steam pressures from 60 to 90 lbs. per square inch, has lately been marked by the extensive adoption in new vessels of the tri-compound engine using steam with a pressure of from 140 to 180 lbs. per square inch. These pressures are not likely to be increased for some time at least, unless some type of boiler is introduced which will not necessitate the present size of casing for containing the steam and water, nor the present method of internal firing, as, with the obtaining pressures, the thicknesses of plates for these portions have reached the maximum capable of being worked on the one hand and of being used for the transmission of heat on the other. The almost universal failures of new types of boilers in the past seem to show that the supersession of the present type of boiler is not at all probable; while, even if a new type of boiler does come into general use admitting of higher steam pressures being obtained, it is extremely improbable, in my opinion, whether steam of the higher pressures would be usable, owing to its high temperature. At a pressure of 180 lbs. per square inch the temperature is 379°, while, if we go to, say 300 lbs., the temperature would be over 420°.

From these considerations it appears to me that if any further great step in economy is to be obtained it must be looked for in the *making* of the steam rather than in the using of it, that is to say, we must turn our thoughts to the boiler, rather than to the engine, which has up to the present, nearly monopolised the attention of engineers.

Undoubtedly there is here a promising field for improvement. It is well known that, theoretically, the heat which may be developed by the combustion of 1 lb. of good coal is sufficient to evaporate about 14 lbs. of water from 100° to 212°; but actually few marine boilers when working at full power really evaporate more than 8 or 9 lbs. of water per lb.

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of coal burned. Suppose that the evaporation of 9 lbs. of water is obtained, there is still between 9 and the theoretical 14, a large margin upon which to work.

In ordinary boilers working with chimney draught we have at the outset a very large amount of heat necessarily expended in producing the draught; but even with chimney draught much might be done in the design of the heating surfaces and funnels to abstract more of the heat from the products of combustion, and still to leave them of sufficiently high temperature to produce the draught required; but when we consider that experience with many ships has shown that forced draught can be applied with almost equal facility to the chimney draught, it is evident that we are no longer compelled to waste heat up the funnel otherwise than by our inability to abstract it from the products of combustion. It is, I believe, by the application of forced draught to marine boilers that the next step in economy will be obtained.

The conditions to be aimed at in the design of steam generators are principally three—

(1.) The most perfect combustion of the fuel must be effected so as to obtain the maximum amount of heat from it.

(2.) The arrangement and extent of the heating surfaces must be such as to be capable of extracting the greatest possible portion of this heat from the products of combustion.

(3.) The construction of the boiler must be such as to ensure its endurance under the conditions to which it will be subjected.

Now as regards the first of these, the theoretical requirements are well known. An amount of air must be supplied to provide sufficient oxygen to completely oxydise the carbon and hydrogen of the fuel, and must be brought into intimate contact with the fuel at a high temperature. In order to supply this oxygen, however, a greater or less additional supply of air must be admitted to dilute the carbonic acid formed, and so allow a fresh supply of oxygen to have access to the fuel.

It appears that the sharper the draught the less additional air of dilution, as it is called, is required, and it is evident that the less air of dilution required, the higher will be the temperature of the products of combustion, because the same amount of heat will be contained in a smaller quantity of matter.

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This question is gone into in detail in Arts. 227-233 of Rankine's *Manual of the Steam Engine*, so that it will only be necessary for me to here quote the following figures from a Table given in Art. 233 :—

Temperature.	Supply of air in lbs. per lb. of fuel.		
	12	18	24
	Volume of gases per lb. of fuel in cubic feet.		
4640°	1551	—	—
3275°	1136	1704	—
2500°	906	1359	1812
1472°	588	882	1176
752°	369	553	738
392°	259	389	519

If the temperature of the atmosphere is 60°, and the fuel is burned by just so much air as contains the necessary amount of oxygen for combustion, viz., 12 lbs. of air per lb. of fuel, the resulting temperature of the products of combustion will be 4,640°. If 18 lbs. of air are used the temperature will be 3,275°, and if 24 lbs. of air the temperature will be 2,500°.

The Table given shows the volumes of the resulting products of combustion at these, and also at lower temperatures; and it is seen that with the smaller quantities of air, not only are the temperatures much higher, but although the products are more expanded on account of their higher temperatures, yet their resulting volumes are also less, while, of course, at equal temperatures their less volumes are still more apparent.

This reduction of volume has a most important bearing upon the transmission of heat from the products of combustion to the water, even if the temperatures are the same, for in a given boiler the less the volume of the gases which has to pass through it in a given time the less will be their velocity; or, in other words, the individual particles of the gases will be in contact with the heating surfaces for a time, longer in direct proportion to the reduction of the volume, and therefore, other things being equal, more heat will be

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transmitted. And since the reduction of volume is also accompanied by a much higher initial temperature, from both considerations, intensity of draught, if accompanied by reduction of air supply, must be conducive to increased efficiency. The influence of the higher temperature will be most marked at the first part of the heating surface, and the reduction of volume at the last part, or tail end of the run of gases.

The correctness of this deduction has been proved over and over again in cargo steamers, with the performances of which I am acquainted, in which, when first built, large grates have been fitted. The grate areas have been gradually reduced in some cases by as much as 20 per cent., maintaining the same evaporation of water, since the engines have been worked at the same speed. The greater rate of combustion resulting from the decreased grate has resulted in higher furnace temperatures and greater efficiency of boiler since the consumption has been reduced to a marked extent.

With regard to the second point, viz., the proportion of heating surface to the quantity of fuel burned, I think that the recent practice of some of our engineers has been retrograde so far as economy is concerned, the proportion of surfaces having been reduced considerably. There is a manifest temptation to do this, as undoubtedly the last portions of heating surface to which the gases are exposed must be, surface for surface, less efficient than the first portions owing to the temperature of the gases being lower, so that a reduction of tube surfaces does not make nearly so great a reduction in steaming capacity as it does in weight and cost. And where a large power on trial trip only is worked for, irrespective of economy of fuel, or where the total weight of boilers is more important than coal consumption, this might be considered to be a good practice; but there can be no doubt that for long ocean voyages, where the coal to be carried forms an important part of the carrying capacity of the vessel, a larger extent of heating surfaces would often mean less total weight of boilers and coal to be carried, besides being a continuous economy in cost of coal and labour of stoking.

As an instance, I may quote the case of some steamers of over 4,500 tons gross register, and regularly advertised as being 4,000 horse-power, built in 1882, in which the total heating surfaces of the boilers bear to the grate area the proportion of 25·8 to 1, while in a similar vessel built by the same firm last year, the proportion is only 21·4 to 1. In very many vessels doing good work the proportion is from 30 to 35 to 1.

That the extent of the heating surfaces may be increased with advantage considerably above that obtaining in the steamers quoted, I will adduce the cases of some other vessels

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in which the boilers were constructed with the proportion 35 to 1, and in which, in addition, feed-water heaters have been fitted in the base of the funnels, bringing up the surfaces to the proportion of 36·7 to 1, without even then reducing the temperature of the gases to an extent incompatible with good draught. This heater has given results, even with the large amount of heating surfaces in the boilers, sufficiently encouraging to warrant their being fitted into several other vessels.

In a later vessel by the same firm the proportion of heating surfaces in the boilers proper to the grate area is 30 to 1, and the feed heaters in this case have been made rather larger, raising the proportion to 32·1 to 1.

In this case I am informed that when running at full power the feed-water becomes raised in temperature more than 40°, while even when the vessel is running at a lower speed with less consumption of coal, and therefore with a less funnel temperature the feed-water is raised in temperature 25°.

If these figures are correct, the saving of heat is quite tangible, and it is evident that the boilers in which the proportions are 21, or even 26, to 1, cannot be expected to be even fairly economical.

I may here remark, that Mr. Howden, in his system of forced draught, places an amount of heating surface in the uptake, in order to absorb some of the heat which would otherwise pass up the funnel, but he prefers to heat the air supply in this manner, instead of heating the feed-water. Although with the same area exposed, the feed-heater must be heavier, both on account of the greater thickness of the tubes on account of strength, and also by the weight of the contained water, yet area for area the feed heater must be much more efficient, not only on account of the specific heat of water being more than four times that of air, but also on account of the much greater weight of the water. Assuming the abstractive power of water and air to vary as the products of their specific gravities and specific heats, water would be about 3,500 times as efficient as air.

If the system of forced draught be introduced, it appears from what I have endeavoured to sketch out, that even with the present ratio of heating surface to the coal consumption, greater evaporative efficiency will be obtained, and therefore the final temperature of gases will probably be lower; but still greater efficiency will then be possible by further increasing the extent of the heating surfaces. This will probably be

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best effected by decreasing the diameter of tubes in proportion to their length, and also by increasing either their number or length.

When the draught has to be obtained by the heat of the gases in the funnel, the ratio of length to diameter of tubes has to be limited, owing to the necessity of not offering too great a resistance to the passage of the gases, and this has led to the general practice of making the diameter of tubes not less than $\frac{1}{24}$th of their length. If, however, forced draught be used, there is no limitation of this kind imposed, and smaller tubes can be used with advantage.

For instance, in a given boiler capacity, taking $3\frac{1}{2}$ in. tubes as a standard, and maintaining $1\frac{1}{4}$ in. spaces for cleaning purposes, as is usual in good practice, we find that with $2\frac{3}{4}$ in. tubes 10 per cent. extra, and with 2 in. tubes 20 per cent. extra surface is obtainable, while if the spaces were reduced to $1\frac{1}{8}$ in. with the $2\frac{3}{4}$ in. tubes, and 1 in. with the 2 in. tubes, which would probably be found to be equally efficient for cleaning when we consider the reduced size of the tubes, these figures are increased to 18 per cent., and 43 per cent. respectively.

I now come to the third part of the subject, viz., the continued endurance of the boilers.

In my judgment experience has shewn us pretty clearly what will and what will not endure the action of the fire under usual conditions, the present boiler, in this respect, being the result of a large amount of experience and of the law of the survival of the fittest. For instance, it is pretty evident that the only furnace likely to give satisfaction is a circular one exposing no riveted seam to the fire, and allowing a small amount of expansion due to variation of temperature without undue straining, a flat sided furnace with its necessary stays having repeatedly been shown to be altogether unsuitable.

Again we know that the present method of fixing the tubes, viz., by rolling them till their expansion puts them in a state of compression in the tube holes, although efficient when the tubes and tube plates are kept clean, will not keep the tubes tight when even a comparatively small amount of non-conducting scale is allowed to accumulate on the tube plates or about the tube ends, the ends of the tubes then becoming so overheated as to relieve the strain they are put to by being rolled; while in other boilers in which a less than usual amount of heating surface is interposed between the grates and the tube plates or in which the tube plates themselves are exposed to the direct radiant heat from the fires

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the same thing happens even when the tubes and plates are clean. It is therefore apparent that so far as tube fixing is concerned, we are about at the limit of successful endurance with our present practice. On this account it will probably be found that, if forced draught is to be successful with a very high rate of combustion, the boiler must be so designed that the temperature of the products of combustion will be reduced before they reach the tube ends. This will not be a difficult matter to accomplish. In other respects I believe the present type of boiler will require no modification so far as endurance is concerned.

We have been much indebted to the Admiralty for taking the lead in the matter of forced draught, and their experience has shewn that the system is quite practicable; but so far I believe I am correct in saying that their experiences have been directed only towards the end they have in view, viz., by burning an excessive amount of coal to obtain excessive power out of a given amount of boiler for a short time, irrespective of the question of economy of coal. This is directly opposite to the direction in which I have attempted to shew progress in economy is to be effected, but as the question of economical cruising is also an important one, it is probable that as some of the vessels fitted with forced draught appliances become brought forward for active service, experiments will be made on them at powers corresponding to, and lower than, those obtainable with natural draught, by working with all the boilers with forced draught, but with much reduced grate areas. If these experiments are carried out and the results made public, we shall all be greatly indebted to the Admiralty, as the results cannot fail to have a considerable influence upon the future progress of marine engineering.

I have received a letter from Mr. Howden, who is unable to be here to-night, in which he points out an inaccuracy in this Paper, as he states that he does not propose to heat the air supply, to the exclusion of the feed water, but that his system provides for the heating of the feed water as well as the air. He has had a multitubular feed-heater partly completed for a year, intended to be put on his experimental boiler. He also takes exception to my figures as to the efficiency of water and air as abstractors of heat.

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DISCUSSION.

Mr. MacFarlane Gray: My Lord, I think that Mr. Milton has written this Paper so well, that there is nothing to say about it. I think that silence is acquiescence in all that he has done. He has done it remarkably well, and I think we should all be very much obliged to Mr. Milton for the clear way in which he has put these remarks of his together.

The President: Silence gives consent. Perhaps Mr. Kirk will have some remarks to make.

Mr. A. C. Kirk: I do not know that I can say much on Mr. Milton's Paper. We are very much indebted to Mr. Milton for it, but, though my experience is of a limited kind, I may say I generally agree in all he says. There is no doubt whatever that an increased rate of combustion on a small grate is more economical. Many experiments have shown this, and for equal efficiency the same amount of heating surface is not required with a high rate of combustion as with a slow rate. If I recollect right, I think Mr. D. K. Clark treats the heating surface requisite as in proportion to the square root of the rate of combustion, therefore, of course, in the same boiler, if you burn the same amount of coal in half the grate, you virtually have a grate with double the ratio of heating surface, more than is required for equal efficiency.

Mr. J. Wright: I have very few remarks, my Lord, to make on the subject. Mr. Milton says the forced combustion practice of the Admiralty has been for the purpose of obtaining power irrespective of economy. That is in a great measure correct, but at the same time, where it has been possible, we have endeavoured, with a view to economy, to increase to some extent the absorbing surface of the boilers. I think that the way in which it should be looked at is the way put by Mr. Milton, that the total weight of the coal, and the total weight of the boiler should be taken into combination with a view to increasing very much the absorbing surface of the boilers if forced combustion is to be used. For instance, the observations we have made lately with forced combustion, show that the consumption per indicated horse power is something between 2½ and 2¾ pounds of coal per hour, more often 2¾ pounds. That is very high, compared with a moderate rate of natural combustion where you will not burn more than about two pounds of coal per indicated horse power per hour. The way to get better economy must first be to increase the amount of absorbing surface, by increasing considerably the size of the boilers. The funnel temperature in our experiments shows that there must be a great amount of waste heat going up the chimney, when it comes to be 1,000 degrees or 1,100 degrees, there is, of course, an immense amount of heat lost. But as I said before, the principal object has been to get a very large increase of power in a short time without caring much for the consumption of coal. Another thing Mr. Milton says, is that it affects the tube ends when the forcing is at all severe. There can be no doubt about that. I take it the line that Mr. Milton would rather go upon would be to create a draught in the boiler by means of a fan, rather than by having a high temperature in the funnel, and by that means get high temperature in the furnaces. This, and a larger amount of absorbing surface, no doubt would very much increase the efficiency of the boiler in the way he indicates. With regard to the experiments which Mr. Milton suggests, no doubt, they would be very interesting, and I hope in time we shall be able to follow

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them out by shortening the firegrates by partly bricking them over, or putting a plate in the ashpit, and so shortening them to the extent of a third or a fourth, or even a half, still using a large amount of air pressure in the stoke-hole, and thereby see what will be the increase of economy from the relatively very much larger absorbing surface in the boiler.

Mr. A. E. Seaton: Mr. Milton at the outset of his Paper touched upon a point in which I feel some interest. He has pointed out, and I think very clearly, that such increased economy will not be got from the higher pressures, as some people expect, for many reasons. Having gone into this question of high pressure steam during the last three years, and in fact having had some experience with it, I can speak with some little confidence. I mentioned last night that the first triple expansion engine made by my firm worked at 110 pounds pressure only, but experience had shown that that ship is very little inferior in economy on a voyage to her sister ship with 150 pounds pressure. The fact is that at 150 pounds pressure a very little leakage becomes a serious loss. Then the higher temperature becomes a serious question. About the only difficulty we have had with the triple compound expansion engine itself has been with the packing of the high pressure glands; it has, however, now been successfully got over by the use of metallic packing, but with all forms of vegetable packing, and also with the asbestos and other mineral compositions we have had a very mixed success. But I have seen quite sufficient in my experience of the triple compound expansion engine to feel sure that very little, if any, practical economy will be derived from increasing the pressure beyond 150 pounds. So firmly do I believe it, that in the engines in which I have been free to use that pressure I have receded to 140 pounds, thinking thereby that the decrease in wear and tear and losses from the sources I have indicated will more than compensate for any supposed loss of efficiency from the lower pressure. It is rather a singular thing, I think, that Mr. Howden should have made the statement that air is a much better means of absorbing waste heat than water. But that may be explained by the fact, that, so far as my knowledge goes all feed heaters suffer more or less from incrustations, and that no doubt a feed heater, although very efficient at the outset when all its surfaces are clean, is apt to become inefficient from the incrustations of the surfaces. I have studied the question of proportion of tube surface and heating surface for the past twelve years, and I may say that at times I have been rather puzzled. The fact is that the coal we use at Hull is South Yorkshire, which is tolerably free burning, so that very little difference in the air pressure or the draught makes a great difference in the consumption. Most of the Admiralty trials, in fact I think all of them, are conducted with Welsh coal; likewise in most of the experiments which have been published by private firms Welsh coal or a similar kind of coal has been used. Now, with slow burning coal you get a very different result to what you get with quick burning coal, and not only that, but you get widely differing results from your boiler, inasmuch as the semi-bituminous coal from South Yorkshire fouls the tubes much quicker than any Welsh coal. Now I have found this failing to be particularly severe in cases where very large heating surface in proportion to grate area has been adopted; in some boilers that have come under my notice the heating surface has been as much as 50 to 1 of the grate, and very often 40 to 1, the economy has been anything but good; in fact I have seen in boilers of from 28 to 30 to 1 very much better results than with larger heating surfaces. I think this arises very much from the fact that with such a large amount of tube surface the tubes get very much more quickly coated with soot and scale, which thereby very much decreases their evaporating efficiency. Our

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practice is to give from 30 to 35 heating surface to 1 of grate area. With the triple compound engine we have gone in for working with very much shorter bars, and we find that the grate is thereby rendered very much more efficient than it used to be, in fact with ordinary chimney draught it is not difficult to get 15 indicated horse power from a square foot of grate. I do not think that a very much better result would be got with a forced draught. I was struck many years ago with the remark Mr. Macfarlane Gray made as to a ready means of computing the consumption of coal in a marine boiler. It was that if you added together the diameter of the furnaces in feet it would give you the consumption of coal in tons per day. I found that was very fairly accurate in general practice, but at the outset I could not conceive how a grate say 6 feet long would burn the same quantity of coals as one 4 feet long; but I think you have only to take a trip to sea and watch the way the fires are worked to understand that. It seems it is very difficult with ordinary firemen to keep a grate over 4 feet long in thorough working order over a whole watch; and in a grate 6 feet long it is very common to find the grate so clinkered up at the back end as to be almost useless. Therefore in many of the older boats we have stopped up the back portion of the grate and got a very much higher efficiency, in other words we have got a sufficient amount of steam to work the engines at the number of revolutions at which they were previously working, and found a considerable decrease in the consumption of fuel. I remember one very particular instance of this, which was immediately followed up by stopping a lot of the tubes, and we found that blocking up the tubes also gave even a better result than the shortening of the grate had done. Therefore I do not quite agree with Mr. Milton that a large increase of tube service necessarily means a large increase in economy. He has mentioned the case of tubes in the back end starting; one very fruitful cause of that is the smallness of the combustion chamber. We have found in boilers where there is an ample combustion chamber it is a very rare thing to find anything wrong with the tube ends, but with the older boilers, where the combustion chambers were somewhat crippled, there was very often considerable trouble; especially with boilers in which the combustion chamber was external and built up of brickwork. The intense heat from the radiation of the brickwork seemed to have a very prejudicial effect on the tube ends. I quite agree with Mr. Milton that our attention should be more turned to the construction of our boilers, and that more especially with a view to economy. Many engineers, I know, are really giving more attention to that just now than to the particular engine; in fact we seem to have got with the triple compound engine as far as we can go for practical purposes, and it remains to be seen what can be done to increase the efficiency of the boiler. I must say that I have hitherto viewed a forced draught with a considerable amount of prejudice, because generally it has been presented to us in the form of a closed stoke hole, and that I by no means like. The panic that is created by a gauge glass breaking in an ordinary open stoke hole is quite sufficient to give an indication of what would happen if the men were all penned in and found a difficulty of egress. I think the system we had pointed out last night by Mr. Robinson a very good one, and one which could be adopted with a considerable amount of advantage. You may have tube surface and tube surface, and you may have a boiler in which there is an ample amount of tube surface, and yet the heat at the base of the funnel is intense; you may have another boiler in which there is less tube surface in proportion to the grate, with an opposite result. That arises from the fact I spoke of, that the hot gases pass too rapidly from the combustion chamber. Take the case of a boiler of considerable diameter having very short tubes so that the area of the cross section of the tubes is very much larger than the area of the funnel; the consequence is that the hot gases

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pass very rapidly from the combustion chamber to the smoke box, not giving the tubes the time to absorb the heat they should have. There is no doubt with a decreased diameter of tube that a very much more efficient heating surface is obtained, inasmuch as you have not such a large volume of hot gases passing through in proportion to the heating surface. There is not nearly so large a volume of hot gas in proportion in the case of a small tube as there is in a large one, but whether it will be found in practice that these short tubes will keep themselves clean I am not prepared to say; with the semi-bituminous coal we have to deal with I am afraid that will not be so. Another thing we find with our marine boilers is the placing of tubes. You may give a boiler a large amount of heating surface, and yet it is not efficient, inasmuch as the tubes very soon get covered and stopped by the deposit of soot in the smoke box; also the gases, when there is a large amount of heating surface, take a short cut to the smoke box, consequently there is not an even distribution through the tubes. I think the question deserves a considerable amount of consideration before deciding upon the proportion of heating surface to grate area.

Mr. M. H. Robinson: Mr. Seaton has referred to a Paper which I read last night upon the subject of forced combustion with moderate draught, and as to the use of small tubes. I am glad to find that the suggestions I made have received so much confirmation from such an excellent authority, as indeed they have from Mr. Milton as well. Mr. Seaton alludes to the chance of fouling the tubes. I can only say that one of our boilers with small tubes was used on the North East Coast of Scotland, and I believe with very smoky coal, but so far as I have heard, it never gave any trouble at all. The tubes there were brass tubes of 1¾ in. diameter, and I think it was in a boiler about 6 feet 6 inches long. The tubes were about 4 feet 6 inches long, or a little more.

Mr. W. Parker: I have abstained from making any remarks on this Paper until I heard what the other speakers had to say, as Mr. Milton and I are in our official capacity so closely connected together. I would like say this: so far as the marine engine is concerned, I think we have gone as far as we can go in the direction of perfection for, at least, some years to come, and that our attention should now be directed, mine particularly, to the boiler, in order to obtain any further reduction in fuel. It will be recollected, that last year, Mr. Howden read a very interesting Paper here, "On the combustion of fuel in furnaces of steam boilers by natural draught and sufficiency of air under pressure," he made some statements which were objected to by two or three of the speakers as being somewhat startling. He stated that by his arrangement of forced blast he could obtain the same practical results by taking out one-half of the boilers, steamships generally had, and in one vessel that he instanced, instead of using twelve boilers, only using six, in fact, burning twice as much coal per foot of grate, as had hitherto been consumed. I for one, doubted that, unless the boilers were so designed that the absorbing power would be equal to the twelve boilers referred to, or in other words, unless the heating surface were very much in excess of the grate surface, and Mr. Milton has endeavoured to shew that in his paper. This, I think, is a fitting opportunity for stating what has been done in this direction. Mr. Howden has fitted a steamer with a single boiler, which had two boilers before. The two boilers had 75 square feet of grate surface, and in the single boiler, the grate surface is reduced to 34 square feet. Forced draught is applied, and the vessel has made a voyage from this country to the West Indies and

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back again, altogether steaming about 35 days. The consumption of coal in the old boilers, which I may remark, were not very well designed to give economy, was some 15½ tons per day, and she steamed nine knots per hour. Now, with the new boiler she steams with the same rate of speed on 13 tons of coal per day, and to all appearance there are no bad effects from the increased temperature in the furnaces, I mean to say that this increased temperature does not appear to be detrimental to the boiler, there were no leaks, and no extra repairs had to be done, and ordinary firemen worked it. Mr. Wright said, "but what about the absorbing surface? When this boiler was made, the intention was that it should have 75 feet of grate surface, so that its absorbing surface would be about the same as it would be without forced blast. I mention this case to show that with a forced blast and a reduction of grate surface, you can at sea get a boiler that will work, and will work satisfactorily under those conditions. I may state also that the space saved by using this boiler is represented to be about 180 tons, but at the same time I must point out that the original boilers were extraordinary boilers with dry combustion chambers, built of bricks, and therefore took up a much greater amount of space than an ordinary boiler would. This saving of space is a very considerable item in a small steamer, still the owner informs me that if they had another steamer to build, they would certainly put in a boiler of this description. I must say, however, that I do not think Mr. Howden's statement is proved yet, as to the very great economy of fuel to be obtained, while at the same time using smaller boilers. I only think he has proved that a forced blast can be used without deterioration to the boiler on long ocean passages.

Mr. Macfarlane Gray: May I say one word, my Lord?

The President: Yes; in explanation of what has been said.

Mr. Macfarlane Gray: Yes, my Lord. Is it not absurd to proportion the heating surface to the grate surface? It ought to be so many square feet per pound of coal to be burnt per hour, or better still per pound of steam of a standard pressure per hour. The grate surface may vary one hundred per cent.

Mr. J. T. Milton: I must thank the Meeting very heartily for the way in which they have received my Paper, and as there has not been any adverse criticisms I need only say very little. I beg to correct Mr. Parker in a little slip that he made, and, although I think he corrected himself, I would like to draw your attention to the correction. He inadvertently stated that this vessel, fitted on Mr. Howden's plan, was fitted with only one boiler of the same size as each of the other two, but he corrected himself afterwards by saying that the boiler was originally designed for natural draught, and that if it had been used with ordinary draught it would have had about the same grate surface as the other two. That, therefore, must have been a larger boiler. The grate surface is 34 square feet. If Mr. Parker had told us the size of the furnaces and the length of tubes we should have seen at once that it is so. That boiler has three furnaces, each 3 ft. 4 ins. in diameter, that is ten feet of width, so that the length of grate could only be about 3½ feet. Now, the length of the furnace tube is 8 feet, and all the smoke tubes were 8 feet long and 3 inches in diameter, so that the boiler has a very much larger amount of tube surface in proportion to the diameter of the tubes than an ordinary boiler would have had, so that it is hardly one that could be compared to a natural draught boiler. Mr. Seaton has pointed out very clearly

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that we have a great many things to consider besides mere tube surfaces. Of course we have. I have not attempted, I think, in my Paper to say that it is a tube surface only to which we must pay attention but when I say tube surface should be increased I mean that as a general rule. There is one remark of Mr. Seaton's which I can hardly understand. He says that blocking up the tubes gave economy by decreasing the heating surface. Now, in some boilers I am very well acquainted with, that same thing happened, but it was not the blocking up the tubes that gave the economy. The tubes were very close together, and when they got dirty they could not be cleaned, and a great many of them had to be drawn to clean the tubes, and instead of renewing them they blocked up the holes and left the space where the original tubes were, so that the others could be thoroughly cleaned; the remaining tubes were kept clean and they gave a better result than the greater number did when dirty. I think that would probably be the case with Mr. Seaton's.

Mr. Seaton: That was not so; the tubes were simply plugged.

Mr. Milton: Probably, then, it must have given a better distribution of draught in the tubes.

Mr. Seaton: I know many instances of it.

Mr. Milton: I think I have no other remarks to make.

The President: You will allow me to convey your united thanks to Mr. Milton for his Paper.

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EXPERIENCE IN THE USE OF THICK STEEL BOILER PLATES.

By W. Parker, Esq., Member of Council.

[Read at the Twenty-sixth Session of the Institution of Naval Architects, March 27th, 1885; the Right Hon. the Earl of Ravensworth, President, in the Chair.]

Mr. Parker: My Lord and Gentlemen, before commencing to read this Paper, I would like to ask you to excuse its brevity and want of detail. The investigation I am about to refer to was only completed last Tuesday, and to my mind it is of so much importance to steel makers and steel users that I thought it would be a pity to allow these Meetings to pass without reference being made to it.

An ordinary cylindrical boiler of 13 feet diameter and 16 feet long, designed for a pressure of 150 lbs. per square inch, for which the scantlings were amply sufficient, burst under the hydraulic test. The pressure was applied very carefully, and when it had reached 240 lbs. the fracture occurred, extending completely across one of the shell plates, and to a slight extent also into the adjoining plate, as shewn on the diagram.

The boiler was constructed entirely of steel made on the Siemens-Martin process by a firm who enjoy the reputation of producing a material second to none in the country.

The plates were all tested at the steel works and fulfilled the requirements of both Lloyd's Register and the Board of Trade.

I find from our Surveyor's Report that the sample from the particular plate which failed—which was 1¼ in. thick—stood a tensile strain of 29·6 tons per square inch with an elongation of 20 per cent. in a length of 8 inches, whilst strips cut from it were bent almost double, cold. In fact the material appeared, from the mechanical tests applied before it left the steel works, to be in every respect suitable for the purpose for which it was intended.

One remark, however, may here be made, namely, that the plate in question was exceptionally large and heavy, viz., 20 feet long, 5 feet 6 ins. wide, and 1¼ in. thick, weighing about 2 tons 16 cwts.

This material was built up into a boiler by a company who have had an unusually extensive experience in the manipulation of steel, having turned out no fewer than 175 boilers of this material.

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The plates were treated precisely as other steel plates have been treated in the same works, and with all the appliances which experience has shewn to be necessary, all the holes were drilled, and the plate was then heated in a furnace and bent to the required curvature in a pair of powerful vertical rolls in the usual manner.

Under these circumstances it appeared at first sight astounding to find the material tearing under a pressure which represents a strain of 6·7 tons per square inch only, or less than one-fourth of the strain which the original sample withstood. In addition to this the appearance of the fracture indicates that the plate did not possess any ductility, stretch, or elongation whatever.

Neither the steel makers nor the boiler maker have as yet afforded any satisfactory explanation of the occurrence.

It is without doubt a most serious affair, especially in view of the high pressures which have now become so common.

On hearing of this accident the Committee of Lloyd's Register instructed me to investigate the matter, endeavour to ascertain the cause of the accident, and, if possible, recommend some measure to prevent such an occurrence in the future. My investigations were only completed last Tuesday, and as such a serious matter as this, which bears upon the safety of life and property at sea, must naturally give rise to no little speculation amongst engineers and steel makers, and has already produced great consternation in many quarters, I have taken this opportunity of laying before you a short statement of the facts as they have come before me, the results of my investigations, and the conclusion which I have arrived at, with a view to eliciting from the various steel makers and steel users here the benefit of their views and experience.

Upon my visit to the boiler-making works I was fortunate enough to find a sister boiler to the one which had burst, ready for testing.

This boiler was tested in my presence to 300 lbs. per square inch, and was carefully measured and gauged and found to show no signs of deflection or yielding.

I also ascertained from an examination of the testing appliances that an abnormal pressure could not possibly have been exerted at the time of the testing of the first boiler.

Seeing that the plates that broke had stood all the mechanical tests required, before leaving the steel works, and that when worked into the form of a boiler shell it gave way at less than one-fourth of its original strength, it appeared at first sight that the plates had been in some way injured, or had undergone some material change from the time they left the steel works until they were riveted into the form of a boiler shell: therefore it became necessary to look carefully into the mode of manipulation of the plates in the boiler shop and especially the heating and bending of them.

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One of the plates was bent in my presence. It was heated in an ordinary plate furnace, but when taken out was far from being of an uniform heat; the end of the plate near the door of the furnace was at a black heat, which gradually increased towards the other end to a dark red heat. Then the plate was turned end for end and again placed in the furnace with a view to heating it as far as possible uniformly, but when again drawn out of the furnace it was seen that the heat was not at all uniform, one end being of a dark red or nearly black heat, which gradually cooled down to a blue heat at the other end.

In this condition it was passed through a set of powerful vertical rolls, and bent to the required curvature. The plate passed through these rolls six times, and by the time the operation was completed one end of the plate was quite cold, while the other end remained at a blue heat.

It was thought that this unequal heating of the plate may have set up in the body of the plate excessive strains of a dangerous character, and that these strains were aggravated by rolling the plate at a dangerous heat, it being well known that the ductility of all steel becomes lessened when worked at a blue heat, and it is, I think, generally admitted that it is far safer to work steel cold, or red hot, than at any heat between these two points. Steel plates, and especially large ones, must be injured by such treatment, but as to the intensity of the strains set up, or their exact locality, nothing definite can be said. To ascertain the nature of the material as it stood, test pieces were cut from the fractured plate, both close to the fracture and apart from it, and subjected to tensile test at one of Lloyd's proving houses, with the following results, which the Engineers have kindly communicated to me.

Samples.	Breadth.	Thickness.	Area.	Total Tons.	Square inch. Tons.	Extension in 8 in. per cent.	Extension in inches.	Contracted Area.
S. I. X . . .	1″	$1\frac{13}{50}$	1·26	40·5	32·14	27·34	$2\frac{3}{16}$	$\frac{11}{16} \times \frac{7}{8}$ & $\frac{1}{32}$
S. C. H. I . .	1″	$1\frac{13}{50}$	1·26	41·75	33·1	26·59	$2\frac{1}{8}$	$\frac{29}{32} \times \frac{23}{32}$
S. 2 . . .	1″	$1\frac{13}{50}$	1·26	41·5	32·93	21·27	$1\frac{11}{16}$	$1\frac{1}{32} \times \frac{13}{16}$
S. C. H. 2 X . .	1″	$1\frac{13}{50}$	1·26	39·5	31·35	23·4	$1\frac{7}{8}$	$1 \times \frac{3}{4}$
S. XX. . . .	1″	$1\frac{13}{50}$	1·26	37·5	29·7	21·8	$1\frac{3}{4}$	$\frac{13}{16} \times 1$
S. IXX. . . .	1″	$1\frac{13}{50}$	1·26	37·25	29·56	26·6	$2\frac{1}{8}$	$\frac{31}{32} \times \frac{25}{32}$
S. XXX . . .	1″	$1\frac{13}{50}$	1·26	38·5	30·5	28·1	$2\frac{1}{4}$	$\frac{11}{16}$ & $\frac{1}{32} \times \frac{15}{16}$
S. I XXX . .	1″	$1\frac{13}{50}$	1·26	38·25	30·3	27·34	$2\frac{3}{16}$	$\frac{29}{32} \times \frac{3}{4}$

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From these tests it appears that the proved tenacity of the plate ranges from 29·5 tons to 33·1 tons, while the elongation ranges from 21·8 per cent. to 28·1 per cent. in a length of 8 inches.

I may say that I corroborated these tests by others made from the same plate for my own information in London—the positions of these test pieces are shewn on the diagram—and they were also corroborated by other tests made for the information of the steel makers.

This range of about 4 tons in the tensile strength of a plate of homogeneous metal like mild steel is very unsatisfactory.

I obtained samples of the plate, and submitted them to five eminent and independent metallurgists, who have kindly furnished me with the results of their chemical analyses, which are as follows :—

Carbon.	Silicon.	Sulphur.	Phosp.	Manganese.
·36	·015	·055	·087	1·05
·27	·016	·044	·076	·641
·33	·010	·038	·065	·612
·30	·018	·044	·063	·648
·26	·005	·038	·067	·650

The most striking feature in these analyses is the large proportion of carbon shewn to exist in the plate. It is particularly high for boiler plates. Material used for thin plates, say, from ½ in. to ⅜ in. thick, to stand the same mechanical tests as these thick plates did, would not contain more than from ·15 to ·18 of carbon; and these facts led us to further experiments.

In view of the great difference in the amount of carbon required in steel for a thick plate and a thin one to stand the same mechanical tests, it was deemed desirable to make an experiment which would determine to what extent work in the shape of rolling, and especially rolling thin plates, which, during the latter part of the operation must of necessity be rolled, comparatively speaking, cold, affected the tenacity and ductility of the material. A slab of steel containing about the same amount of carbon as the plate that ruptured, viz., ·33, was obtained at the steel works where the plate was made, and rolled at one heat down to ½ in. in thickness. This material, had it been rolled down to 1¼ in. plate, judging from the carbon it contained and the tests of the broken plate, as well as the opinion of the steel makers, would have had a tenacity of from 30 to 34 tons per square inch. It was found, however, that when rolled down to ½ in. thick its tenacity was increased to from 35 to 41 tons per square inch, with an elongation of from 21 to 24 per cent. in a length of 8 inches.

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Other pieces were made hot and quenched in water. These, when tested, broke at a tenacity of from 44 to 45 tons, and had, practically speaking, no stretch at all.

Pieces were cut from the fractured edge of the plate, as shewn on the diagram, and subjected to tensile, bending and temper tests.

They showed a tenacity of 33·5 to 34·2 tons per square inch, but they stretched only 13 and 16 per cent., and broke with a crystalline fracture, as will be seen by the specimens produced. They bent cold to a considerable degree, but when made red hot and quenched in water, instead of bending, as pieces of a thin plate of similar tenacity and ductility would do, they broke under the first blow of a hammer without any bending whatever. The material was so high in carbon as to take a temper and become quite hard and brittle.

Further cold bending tests were made from pieces of the broken plate, both before and after being annealed: those which were tested before annealing bent fairly well, strips 1¼ in. square bent to an angle of 49 and 61 degrees, the fracture shewing a considerable amount of alteration in form; while those pieces which were tested after annealing bent much better, in fact, almost double. Strips, however, that were heated and quenched in water broke short without any bend whatever at the first blow of a hammer, and thus corroborated the previous experiments made in London.

These experiments point to the fact that the plate which gave way must have become partially tempered by the heating and cooling to which it was subjected for the purpose of rolling it into its cylindrical form. The heating not having been uniform, the tempering could not have been uniform, and the variations in the temper no doubt have caused the variations in the strength and ductility shown by the different parts of the plate. The hardest part of the plate yielding less than the rest became naturally more strained and hence the plate tore at its hardest part at a pressure only a small fraction of that which it would have borne if its yielding had been uniform.

Having thus placed before you the nature of this accident, and the steps taken with the view of unravelling the supposed mystery, I now venture to state what inferences may, in my opinion, be drawn from the results of the investigation.

I think it will be acknowledged that a material which is so high in carbon as to take a temper and break short as described, even if it possesses high qualities of tenacity and ductility before being tempered, must be looked upon as unreliable and altogether unsuitable for use in marine boilers.

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It would appear that the desire to obtain high steam pressures, and to use steel of a higher tenacity consistent with a large amount of ductility, has caused the marine engineering world to unknowingly drift into using a material of an unreliable and unsuitable character for the shells of marine boilers, more especially when the usage which such plates receive in heating and bending is considered, for, except among steel makers, it does not appear to have been generally known that the thicker a plate is, the more brittle and erratic in its behaviour it must become, as compared with a thin plate made to stand the same mechanical tests as far as tenacity and ductility are concerned, as, otherwise, I feel convinced that the increase in tenacity from 29 to 32 tons for thick boiler shells would not have been advocated.

So far as I am concerned, and the Society which I represent, I may say that it has always been our endeavour to discourage the use of steel of high strength. The rules of Lloyd's Register require boiler plates to have a tensile strength of from 26 to 30 tons, and have done this from the commencement of the use of steel, because we felt that the higher the tenacity arrived at, the more likelihood there would be of the plates giving trouble, and our whole desire has been to keep the material mild. We have, however, had considerable pressure brought upon us by manufacturers and engineers to allow a strength of 32 tons per square inch for thick boiler shell plates.

This accident and the investigations which have followed, clearly point out that engineers have been drifting towards the use of an unreliable material, or at all events a material which is too near the verge of danger to be pleasant, a state of things that should not exist with steam boilers.

I would therefore urge, in order to remedy this growing evil, that the tenacity of steel plates for boiler shells—which are becoming thicker every day, should in no case exceed 30 tons; and that a temper test should be insisted on from *every* thick plate, and the practice of using enormously large plates should be discouraged; while more care should be exercised in uniformly heating and bending these plates.

I have conferred with the principal steel makers in the kingdom on this subject, and am able to say that they agree with me, and are decidedly of opinion that steel plates over an inch in thickness, and having a tenacity of more than 30 tons, must contain so much carbon as to render them unsuitable for boiler-making purposes, although they may possess the necessary tenacity and ductility to withstand the usual tensile and cold-bending tests.

I venture to hope that this Paper will be made the subject of a discussion, with a view to obtaining further opinions respecting the important points in question.

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DISCUSSION.

Mr. W. Boyd: My Lord, though I have not the honour of being a member of this Institution, yet, as having built the boiler which has been reviewed in this Paper, it occurs to me that it might be suitable that I should open the discussion here this evening, the more so as my friend Mr. Parker has, perhaps, to some extent—I have no doubt unintentionally—seemed to reflect upon the boiler maker and the steel maker in that they have not solved this problem for him. Mr. Parker in his Paper says, that the steel maker and the engineer "have no explanation to offer as to the cause of the accident." Well, the steel maker must speak for himself; as far as the boiler maker is concerned, I have only to say that that is the case, and my only object in troubling you for a few moments here to-night is to say that, as far as we are concerned, our only desire is that the subject should be fully discussed. We have no wish to keep back anything that can be brought to bear on the mode in which the boiler was constructed, and if we are able to offer any information which can be thought to throw any light upon the subject, we shall only be too happy to do so. Mr. Parker's Paper has described the mode in which this boiler had been treated and had been built, and he has done it so clearly that I do not know that there is any important point to which I can add anything. Reference has been made to the furnace in which this plate has been heated. I would only say that this furnace is of the usual type, such as is common on the North-east coast. It is a rectangular furnace, 21 feet long, by 7 feet 6 inches wide; it has a firing place at one end, at the opposite end to the door, at which the plate was admitted and removed; and two other firing places, one at each side, placed in echelon, so that the method of heating the plate that was adopted in this case was such that, until the last week or two ago, it was thought to be sufficient, and was accepted as satisfactory and usual. I think, Mr. Parker perhaps a little exaggerates the appearance and the different temperatures of the plates as they passed through the rolls, particularly when he says that one end was quite cold after passing through six times. Now, I have seen numbers of these plates bent myself, and though, of course, the temperature does fall very considerably, I think it is rather a stretch of words to say the plate was "quite cold." The rolls on which the plate was bent are immediately adjacent to the furnace in which it was heated. The plate is brought out horizontally, is upheld by an overhead crane, and run through these vertical rolls, so that almost the minimum of time is taken in the transfer from one place to another. With regard to the boiler itself, Mr. Parker has already given you a table of tests, and the places from which they are taken. As I said before, I shall be very pleased, indeed, to answer any questions that may be raised to-night as to the mode of construction of the boiler. There is only one other thing that occurs to me to mention in connection with it, and that is that the seam on the left-hand side of the diagram, with the double row of riveting, is the last seam in the construction of the boiler. That seam is closed by hand after the end plate is put in, and after the furnaces have been put into their place in the boiler. So that those rivets that are on the left-hand side, being hand riveted, are to some extent under a different condition from those in the treble-riveted seam, and in the cross seam, and in the butt strap, which are put in by machinery. I do not know that I have anything more to add.

Mr. Colville: My Lord, I wish only to introduce our engineer, who will give the data with regard to the plate which failed. I am here as the representative of the firm who have manufactured the plate that

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failed; they have made tens of thousands of tons of boiler plates. The Board of Trade and Lloyd's know our material very well. We have certain facts to put before the Meeting as to the heating that will throw a great deal of light on the subject. Our engineer, Mr. Cuthill, has a certain theory with regard to it.

The PRESIDENT: I am very sorry to interrupt you, but I must put it to the Meeting, because this is not entirely in accordance with our rules. A discussion is one thing, but this is practically appending a Paper by reading this statement. I am not anxious to interfere, but my duty is to see that our rules are observed, and I must take the sense of the Meeting whether they will hear you. If Mr. Cuthill would favour us with any speech he likes, of course we will hear him; but I am afraid I cannot allow a second Paper to be read of which no notice has been given.

Sir EDWARD REED: I beg leave to move, that under the peculiar circumstances of the case, and owing to the great importance of the subject, Mr. Cuthill be allowed to make his statement in any way that may seem suitable to him.

Mr. MACFARLANE GRAY: If he keeps within his time, I think we ought not to object to his reading it.

Mr. W. CUTHILL: I am engineer to and represent the manufacturers of this unfortunate plate. The subject being of the utmost importance to both manufacturers and boiler makers, I trust to your kind indulgence in being allowed to make the following remarks:—

In reference to the author's paragraph at the close of his Paper, regarding the steel maker's views coinciding with his as to the desirability of ceasing to make and use this high strengthed steel for boiler shells, I have to explain on behalf of the firm I represent, that they agreed with him on the point in consideration of the evident difficulty experienced by boiler makers in manipulating such high strengthed steel in plates so large in size and thickness, believing, however, that the cause of failure is not so mysterious as it looks, and that this difficulty of the boiler makers may be easily overcome—all as I hope to show, they wish their agreement with these views thus qualified.

I saw the plate immediately after its failure, and certainly in appearance a more mysterious fracture could not well present itself—not complete to the one edge of the plate, and only through to the other by a fine hair line, it gaped and bulged out of line at the centre in the most ugly manner. Its manipulation in the boiler yard was freely and obligingly described.

As to the history of its manufacture, the steel was made in the usual most approved manner from the purest materials by the Siemens' process, cast into a 4-ton ingot measuring 30 ins. by 21 ins. at bottom and 19½ ins. thick at top. It was again reheated and reduced under a 12-ton steam hammer to a slab about 10 ins. thick, weighing about 7,000 lbs. It was again reheated and rolled to thickness, the plate when shorn measured 20 ft. 7 ins. by 66⅜ ins. by 1¼ ins. thick, weighing about 2¾ tons. Its final stage was a careful heating equally all over in an improved plate furnace, and allowed to cool in the open air.

The test pieces were shorn from the scrap of the plate and underwent the same process of heating as the plate. The tensile and bending tests of this particular plate were made in the presence of both the

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Board of Trade and Lloyd's surveyors. The test stamped upon the plate being 29·6 tons, and 20 per cent. extension in 10 ins.

Some of the tests since made of the plate corroborate the original, while others are considerably higher, averaging about 2 tons over it. No explanation can be given as to this other than that the original was taken from the scrap edges, while the higher ones and better in extension have been taken from the plate. No circumstance in the manufacture can account for this variation, which in any case cannot explain the cause of failure.

The reliable analyses give the elements of a good quality of steel for this thickness and strength of plate, the carbon is somewhat high and is proof of the otherwise pure quality of the material—considering the behaviour of the steel under mechanical test it is evident to any steel maker that several of the analyses given are quite unreliable. Taking the last statement of the list as correct, it is not to be wondered at that the steel tempers to brittleness when cooled from red heat in water at winter temperature. Under no possible circumstance could shell plates be subjected to this treatment either in the manufacture or life of the boiler, unless in the case of the ship getting burned down, and after all was beautifully hot the shell suddenly dipped in the cold water on its way to the bottom. Several of the test pieces supplied to my firm were annealed before testing, and showed that no tempering of any degree had occurred in the boiler maker's hands.

Carefully considering the whole matter in view of finding the probable cause of this remarkable failure, I maintain nothing will explain it that can be found in the composition, manufacture, tests, tempering, quality and general condition of the plate as it left the manufacturer's hands.

My theory as to the fracture of this plate is just the one in my view that explains all fractures occurring in steel plates of good quality, whether they be of high or low tensile strength—low or high in carbon—and is consequently well worth the best study of steel boiler makers generally, as it is easily under control.

When a plain steel plate is locally heated and simply allowed to cool again, no harm will result. No plate was ever known to crack from this treatment; but while under this local heat, were work put upon it so as to change its form, or disturb its molecular arrangements in part or whole, then internal strains are set up, which according to their severity may or may not set up cracks. When this internal change due to the work put upon the plate is completed, its condition so far as the local heat is concerned seems to become normal, the work having acted as a lock upon unequal contraction, all the contraction that otherwise would then take place from that unequal heated condition throws up strains between parts.

Coming to the plate in question, it was heated previous to rolling to the shell curve in the usual type of boiler maker's furnace, with a large grate at one end and two small side ones somewhat according to sketch.

I did not see the plate heated, but judging from the manner in which these furnaces act upon large plates, the plate would be heated something like the shading on Fig. 5, Plate XV., representing the plate drawn to scale, and showing position of crack. Now, when rolled to the circle, the whole material of the plate would be disturbed in its molecular condition, that condition becoming normal with this variety of heat upon it, severe strains are certain to have been set up internally in cooling. As evidence, several plates had been observed to be curved across looking on end, thus
So severe had these strains been in this particular plate, and that particular part of it, that they only

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required the assistance of some 7 tons test pressure per square inch, to overcome the strength of the plate.

Some idea of the amount of strain caused in this way, might be obtained by comparing the extension of steel per ton per square inch, up to the limit of elasticity, with the contraction of steel per degree of difference of heat, thus—

By Kirkaldy's test on mild steel, this extension is ·0000802 of its length per ton per square inch.

Box's *Treatise on Heat* states that iron contracts or expands ·00000669 of its length per degree of difference of heat. I find that steel is in excess of this by about 8 per cent., take it at ·000007.

You will now see that every 12° of difference of heat causes as much contraction as one ton of tension per square inch will extend steel. Again referring to Box's Treatise, different temperatures are given for different colours of heat, thus—

		Difference.
Red just visible, 977° Fahr.		
„ dull 1,290° „	. . . . .	313 ÷ 12 = 26 tons.
„ cherry dull 1,470° „	. . . . .	180 ÷ 12 = 15 tons.
„ „ full, 1,650° „	. . . . .	180 41 tons.

dividing these differences by 12°, the corresponding stress is as shown above.

Along with this, the nature of steel should be considered; its homogeneous and finely crystalline structure, and its evident preference to cracking instead of stretching, when under this internally bound up condition, which, however, can be effectually corrected by equal re-heating.

This may not explain as a demonstration of Euclid how the plate cracked, but to my mind, it considerably takes the mystery out of it, providing my supposition as to the heating by the furnace is correct.

As one who has had long experience in the manufacture and use of steel, I would greatly regret to see our good friends of Lloyd's registry, and the Board of Trade, putting their feet upon this class of steel in question.

In my opinion as an engineer, steel as made by the best reputed makers, up to 33 or 34 tons, is in every respect perfectly as reliable for boiler shells or bridges, where no local heating is necessary, as steel of 26 to 30 tons strength is for internal parts of boilers.

But we must make a servant of it, and it will be found a thoroughly reliable and good one, and at once set about improving our means of heating large plates.

An improved design of plate furnace, Figs. 3 and 4, Plate XV., is strongly recommended to boiler makers, it is open at both ends, with a series of grates, small at the centre and large at the ends on one side, and a corresponding series of dampers in the flues leading to the chimney on the other. My firm has long ago discarded the old design in preference to this, with the most satisfactory results. There is no patent about it, any one can adopt it.

Mr. W. Denny: Mr. Parker's Paper divides itself into two parts, the first a very careful description and equally careful analysis of this accident, the second a recommendation as to what changes in practice should be made in consequence. We should thank Mr. Parker, as for similar work we have had to thank him in the past for the first part, and we may thank him unreservedly for it. In accepting our thanks,

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Mr. Parker can permit us to differ somewhat from him as to the latter part of his Paper. Now, the first remark which would occur with regard to this accident is, that "one swallow does not make a summer," and that one cracked boiler plate does not afford a sufficient reason for a panic. Another reflection which suggests itself is, that we are very fortunate in having a hydraulic test for boilers by which it is possible to discover such weaknesses as the one we have just had described to us. It would be a pity if the hydraulic test were to be used for ever without demonstrating its value, although I hope it will exhibit this as rarely as possible. Mr. Parker has told us that according to the steel-makers, and I wish they had communicated the fact earlier, there are certain inevitable and objectionable peculiarities in the nature of thick steel plates. But if these are inevitable peculiarities, the question rises whether a diminution of the tensile strength from 32 to 30 tons is likely to cure them. If the peculiarities are inherent and absolutely objectionable, something more than this would be required. What we shall really find is, that the full discussion which will be this night, and after it, given to this unfortunate plate, will have the effect of directing the attention of makers to the necessity of improving these thick plates, and they will find ways of doing this. When steel plates were first used for shipbuilding, we had many defects in them, but I think all shipbuilders who are now using steel, will concede that these defects have practically disappeared. There is no reason to believe that the men who have conquered the defects in steel plates for shipbuilding, will not conquer the defects in steel plates for boiler-making. Therefore I say, we should not legislate in a panic about this matter, but discuss it fully and calmly and delay making any recommendations until the discussion has been as full and complete as it ought to be. Speaking as a shipbuilder, I sincerely hope this will not have the effect of turning backwards and downwards the tensile strength of plates for shipbuilding. Everyone who has the interest of shipbuilding at heart will wish that we may obtain the fullest advantage from the use of steel, and the fullest advantage is to be obtained by lightening our structures and not by increasing their weight. But if they are to be lightened, we must have stronger or, at least, not weaker steel than we are at present using. My firm has had some experience in the use of a stronger steel than Lloyd's Society permit—I mean the steel sanctioned by the underwriters' registry. We have used this steel in a very large steamer of nearly 5,000 tons gross, and we have had quite as much satisfaction with it as we have had in the use of the milder material. I shall not further detain the meeting. My only wish has been to ask it to pause in the consideration of this matter, to look at it coolly and calmly, but with the determination to overcome its difficulty.

Mr. A. C. Kirk: My Lord and Gentlemen, Mr. Denny, in the opening part of his speech, has said very nearly the very words that I had in my mind to say. We are certainly much indebted to Lloyd's Registry and to Mr. Boyd, for the full information they have given us as to this very interesting case. I say it is very interesting, all failures are interesting, and all failures are instructive, but we are not going to call it alarming. This is not the first time a steel plate has cracked, and it is not the first time a steel plate has cracked and we do not know why. A few years ago, I described before this Institution an accident which occurred in our own practice to a tube plate which cracked upon getting a severe blow from a hammer, on a centre punch. I will not say more about it because it is recorded in the Transactions of this Institution. Before that, while the *Comus* class was building at Fairfield, a steel skin plate behaved in the same fashion. That plate was a Bessemer plate. The tube plate was a Siemens plate. They

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were both soft plates as we should call them. Here we have got a Siemens plate that is a hard plate, and it has cracked too, therefore, if we simply put together these few facts, the cracking seems to have no earthly concern with whether it is hard or soft. Now, my friend, Mr. Dunlop, puts me in mind that in his neighbourhood, the steel front-plate of a boiler cracked. Now, one peculiarity of all these cracks, this one included, is, that where the fracture takes place, there is no reduction of area. Now, if the crack is produced by mechanical tension, there will be a reduction of area. In a little Paper (Vol. XXIII., page 131) about the tube plate which cracked, I gave, I think, if I recollect, the drawing of a piece of plate that I had pulled at the Glasgow testing-house till it cracked; the plate thinned out to a very great degree, but I could not make the crack in the tube-plate extend any further than it had cracked of its own accord; I wedged the plate open at the edge, it would not go further, that was no good, it went its full length instantly, and further it would not extend. The surface of the crack was bright and crystallised; the tensile and bending tests taken from the very side of the crack, were perfectly satisfactory in every way, it was only along a line that this weakness and defect occurred. I expressed the opinion then, be the cause what it may, the probability was that the origin was in the ingot itself. Now, these cracks are very, very rare. I saw a set of boilers, by the way, made of French steel, soft steel too, and there were cracks in the same fashion. Upon these grounds, I see no reason whatever to be in the least degree alarmed. Here a very exceptional thing has happened. As Mr. Denny says, "one swallow does not make a summer;" the bad plate has been found out by the hydraulic test, and there is an end of it, and of all alarm concerning it. There is one thing that Mr. Parker says here as to the thick plates, and the quantity of carbon in them; I do not see why an engineer or a boiler-maker needs to concern himself with the amount of carbon in the shell plates. I say "shell plates," mark, not the ends of a boiler, the shells of a boiler might just as well be of cast-iron, if they were thick enough. Our steam cylinders are made of cast-iron, and the shells of the boiler are not exposed to more strain from the steam, or so much from unequal expansion as the cylinders of steam engines are. That reminds me, that cast-iron cylinders have cracked sometimes, but I do not think anyone is alarmed, or proposes to make them of anything else all the same. Now, just the day before I left home, an eminent steel maker was talking to me about this very thing, and he was telling me how it was necessary to put more carbon into a thick plate than into a thin plate, to get it up to the same strength, for, he said, as the plate gets thicker, you put less work upon it, or at any rate, the work does not reach the centre of the plate so well; thus, I suppose, we are rather between the devil and the deep sea. We must either make our plates stronger to have them thinner, or we must have them thicker and have less work upon them, and they will not be so good. I do not know which of the two things is best to choose. It is only fair to say that the plates turned out of Mr. Colville's works, and I have worked a good many of them, are as good steel as is to be had in the country. Mr. Parker says, that when the plate was rolled down to ½-inch thick, its tenacity was increased to from 35 to 41 tons. Now that seems to me a very wide margin. I suppose it means that the tenacity was something between 35 and 41 tons, perhaps Mr. Parker will explain that. I have said before, that these remarks apply simply to the boiler shell, for the internal parts of boilers we cannot certainly have too ductile a material. Now, as to this steel being hard, and as to its tempering, which has been put forward as something very awful and dreadful, I do not see that, because the steel can be tempered, that has anything to do with whether it is applicable to the shell of a boiler, or whether it is not, for this very excellent reason,

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that it is never going to be tempered, it is never going to be used in that state. Now, as to what Mr. Colville said about the steel being in a state of unequal tension. What I said before,* I think dissipates that entirely. If the fracture was a purely mechanical fracture, we should have had this steel drawn out to its proper and normal amount of elongation during fracture. I had a little experiment made with very hard steel, I ordered it just before I came away, in view of this meeting, to show that after all high carbon steel is not such dreadfully delicate stuff as one would think. I had a piece of octagon chisel steel turned down to half an inch diameter, leaving a $\frac{7}{8}$th screw at each end, as a test piece, the body being five inches long between the nuts. The body of that was heated to a cherry red heat—a dull red heat; it was put through a piece of cast-iron, and a nut screwed tight up at each end of it, and left to cool, the result was that in five inches, when taken out nearly cold, there was a permanent extension of nearly $\frac{1}{16}$th of an inch. It was repeated, and there was a further extension of nearly $\frac{1}{16}$th of an inch, and I am only sorry that in my absence the experiment was not continuously repeated, so that we should have seen when the steel would have broken, if ever it would.† Of course, when you take chisel steel, that is taking a most extreme case. With reference to a remark made by one of the speakers, I propose to say a word. What I said before about these other examples of cracked plates, proves that the cracking of plates in the way I spoke of, is not confined to thick plates. The first case of the skin plate of the *Comus* class, perhaps Mr. Barnes will correct me, was, if I recollect right, under half an inch thick. The tube plate was a $\frac{3}{4}$-inch, and we have now an inch and a quarter plate doing the same thing.

My Lord, I think I have said all that occurs to me.

Mr. MacFarlane Gray: My Lord, Mr. Kirk has very aptly set this dilemma before us. There is certainly some yet indefinite evil thing operating in these thick plates, and Mr. Parker is well qualified to find that evil thing; he has pointed it out where he thinks he sees it, and he invites steel-makers and boiler-makers to co-operate with him in casting it out. While frankly giving Mr. Parker full credit for the thoroughly practical way he has set about this investigation, and the intelligent report he has kindly communicated to us, I am inclined to reserve yet my acquiescence in full to his conclusions. That the material was of a quality capable of being tempered he has proved by actually hardening test pieces, but I do not think it was in that hardened condition in the boiler when the plate was ruptured. I come to this opinion from observing that the test-pieces taken from the fractured plate, close to the fracture, before being annealed all bent fairly well cold, the fracture showing a considerable amount of alteration of form. I cannot, therefore, assent to the statement that in the fracture we "find the material tearing under a pressure which represents a strain of 6·7 tons per square inch only, or less than one-fourth of the strain which the original sample withstood."

Then with regard to the unequal heating of steel plates, and its effect upon the steel; that, perhaps, is not so fully understood as it should be. If you wished it, for experiment, you could produce a similar fracture; that is, if you take a plate and heat a part of it but do not heat the edges, that part which is heated must expand; but the edges of the plate do not allow it to expand; the plate is then bound to

* See Vol. XXIII, page 131.

† Since the Meetings this experiment has been repeated, in all, thirteen times. The total extension, due to contraction in cooling was $\frac{11}{16}$ inch, and the diameter was reduced from $\frac{1}{2}$ to $\frac{7}{16}$ inch. The bar was not cracked or broken.

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increase its volume, and does so by stumping itself up, that is to say, it thickens itself at that place; then it cools down, and when it cools down the place that stumped itself is too short for the place it is in; we do not see it, and then it is bedded in.

I think that no part of that fracture was the result of less than 26 tons stress per square inch. I think that in some way, either by unequal heating of the plate or by unfair construction of the boiler, the part which gave way first had more than its fair share of the tension, while other parts of the shell, either in that plate at the section of rupture, or in the adjoining course of plates at that place had little or no tension on it when the rupture started. Did Mr. Parker cut and plane a section of the rivets in the adjacent circumferential seams to ascertain, not the fairness of the holes, but whether the rivets showed that one course of the plates had been normally in high tension and the other in high compression at the place of rupture. If this were so, the second course of plates may have been doing nothing towards the strength of the shell at that place at that time, and may only have been relieved of compression stress by the fracture of the first course. The position of the fracture and its stopping just before the end edge would lead me to think that the fracture commenced at the seam between the two courses of plates, where, perhaps, the riveting up of the circumferential seam finished, and that the plates having been unfair before riveting up, there would be tension on the one and compression on the other. On this hypothesis the fracture may be explained without supposing any deterioration of the plate such as Mr. Parker fears did exist.

Mr. F. W. Dick: My Lord, I am also a steel maker, and as this question of fracture has caused some disquietude, I may be excused for speaking upon the matter. The Steel Company of Scotland, with which I am connected, has had, perhaps, more experience in the manufacture of steel boiler plates than any other firm existing. We have noticed, for the last two years at any rate, a growing tendency among engineers to increase the thickness and size of their boiler plates, because, I suppose, of the increased boiler pressures which are being used. Steel makers are very seldom consulted about what should be done or what should not be done. We are simply asked whether we can supply a steel plate to stand certain tests and certain conditions. If we can do so we get the order. We have supplied several hundred tons of these thick plates, and in no instance have we had any failure with them. We have had no fear of their suitability for use in boilers, but nevertheless this failure has occurred. Mr. Parker has given two reasons for the failure of this plate, Mr. Kirk has also given a reason for its failure, and Mr. Cuthill has said that he has no doubt that the plate was in perfect condition when it left the works. I am inclined to think it was. I cannot conceive that any initial weakness could exist in an ingot which would not be noticed before the plate left the works. If the plate when it fractured showed a clean crack the ingot must have been in a similar condition, and that the defect arose from an initial defect in the ingot I do not think in the slightest degree likely to be the case. Then as to the want of extension in the fracture. I must say that I do not know exactly how to explain that, but I have seen test pieces 8 or 10 inches long, and broken slowly, show most excellent extension, while the same material, fractured suddenly, breaks off perfectly short without any appreciable extension. Then, Mr. Parker has spoken about the blue heat and also about the thickness of the plates. I can say a little on both those points.

It was mentioned that the plate was certainly hot when it was being punched, because you could not bear your hand on it. Now this is, in my opinion, the very essence of the whole danger. If the plate

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had been red hot there would have been no danger. If the plate had been cold it would have bent without the slightest fracture, but because it was in such a condition that you could not see it was hot, but at the same time you could not bear your hand on it, it was in a very dangerous condition, indeed. If a piece of mild Siemens' steel be heated to what we call the blue heat, that is to say, to a temperature of about 250 degs. Fahrenheit, it is in a very peculiar condition. If you attempt to bend a piece of plate in this condition it breaks when it reaches about a right angle, that is, when it is bent to about 90 degs. If you make a tensile test at this temperature you will find the tensile strain raised from two to three tons on the square inch, and that the extension is diminished about 10 per cent. If you go beyond that temperature, and go above 250 degs. Fahrenheit, you will find that the breaking strain decreases, but that the extension increases while at temperatures between the blue heat and the extreme cold, you will find the extension is scarcely altered while the breaking strain is slightly increased. I have made many hundreds of experiments at many different temperatures, ranging from several degrees below freezing point up to red heat, so that I speak from actual practical knowledge on this matter. Now it was shown that this plate was unequally heated. I do not know to what extent unequal heating in plates may distress them, because we know that in everyday practice plates are unequally heated, and yet stand. That is seen especially in plates which have to be flanged, and especially in back tube plates where there is a lot of hand work in the flanging. If a piece of steel is heated to blue heat, and allowed to cool down without anything being done to it, there is no harm done, the steel is in as good condition as it was before it was heated, but if the steel is heated to a blue heat and has work put on it and then is allowed to cool it will be in a very different condition. I have no doubt that the fractured plate was put in in that condition—that is to say, it was bent, and put in the boiler straight off, and having been unequally heated the plate was strained at the critical temperature at this blue heat. Now about the thick plates. We are, to some little extent, between the devil and the deep sea with regard to thick plates, for the following reason: A steel plate half an inch thick, to stand a tensile test of, say, 27 tons to the square inch, would require about 0·15 of carbon per cent. If that piece of steel were rolled into a plate of 1¼ inches thick it would probably break at about 25 tons per square inch. On the other hand, if a piece of steel 1¼ inches thick is wanted to stand 32 tons per square inch, in my opinion it must have carbon from 0·22 to 0·24 per cent. A plate with, say, 0·22 of carbon per cent. rolled down to a thickness of ½ or ⅜ of an inch would break at something like the strain the fractional plate showed, somewhere in the region of 40 tons to the square inch. From this it will be evident to you that very small variations in the proportion of carbon in a plate cause very sensible differences in its breaking strain and in its behaviour. Ordinary mild steel, such as we use for boiler furnaces, contains from 99¼ to 99½ per cent. of pure iron. It is evident that if the steel had 100 per cent. of pure iron that we should have a very much less chance of variation, that chemically considered the metal would be perfectly homogeneous, and the only chance that we should have of destroying the homogeneousness of the plate would be from physical causes, that is to say, from such causes as unequal cooling, and even then unequal cooling would be much less likely to affect the plate. Therefore, if you increase the carbon in a plate in order to increase its tensile strength, you increase the quantity (I will scarcely say of the impurities), but of the material which is not pure iron. You therefore increase the chance of variation in the amount of distribution through the plate of these materials. Therefore the variation of 4 tons per square inch which was observable in the plate is easily accounted for and is a thing which may or may not happen in high carbon plates but

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which is likely to happen, and it is a thing which you must take into account. Just one more point with regard to the rolling of a thick plate. It has been mentioned that in rolling thick plates you could not get the same work upon them, that is to say, in rolling metal down to a thin plate, say to $\frac{3}{8}$ or $\frac{1}{2}$ an inch thick, the molecules, if they have long dimensions, seem to have their long dimensions laid closer together, their centres of attraction are in the closest possible position, and therefore the plate has a higher tenacity for the same material. In a thick plate it is impossible to obtain this closeness, although I must say I have not been able to detect by careful experiments any difference in the density between a thick and a thin plate, still the appearance of the fracture is different. We usually roll an ordinary plate from an 8 inch slab. It has been said, if you are to roll a thick plate why not start with a slab, say, 20 inches thick. That makes no difference. It is because it is so hot when finished and so thick when finished that the pressure does not seem to be transmitted to the centre of the plate, we have never found any danger following the use of these thick plates, but certainly know that plates under 1 inch in thickness give better test results than plates of over 1 inch in thickness.

Mr. Raylton Dixon: I should not have presumed, especially at this late hour, to have detained the Meeting with any remarks of mine, but having listened with great interest to Mr. Parker's Paper and the remarks of those gentleman who have spoken, and the reasons given or the suggestions made by Mr. Parker and by others, one idea has occurred to me which has not been ventilated, and although I would, of course, rather submit to those more practical gentlemen with regard to the rolling of steel plates and the working of them, still I think it is advisable that any solution that can be propounded as to this accident or difficulty should be considered. I notice Mr. Parker assigns the cause of this fracture to the heating of the plate in the furnace where the boiler was made. To quote his words, "These experiments point to the fact that the plate which gave way must have become partially tempered by the heating and cooling to which it was subjected for the purpose of rolling into its cylindrical form. The heating not having been uniform, the tempering could not have been uniform, and the variations in the temper no doubt have caused the variations in the strength and ductility shown by the different parts of the plate." On the other hand Mr. Kirk and Mr. Dick, or rather Mr. Kirk I think it was, suggested that the fault was a more radical one in the ingot of which the plate was made, but neither of them has suggested or entertained the idea that the fault could have been caused in the conversion of that ingot into the plate. It has occurred to me from some similar matters that have come under my notice, that it is not improbable that the weakness has been caused in the very way that Mr. Parker explains, not in the irregular heating of the plate in the boiler-maker's furnace, but by the irregularity of heat or the sudden local cooling that might be produced during the rolling, especially of a heavy plate, by its stoppage for a short time by any hitch in the rolls. I believe that a plate passing through the rolls in that way, if anything occurs to stop its progress, then, no doubt, there is on that part of the plate on which the rolls are bearing, a sudden reduction of the heat in a line *across* the plate, and most of these heavy plates have been found to have gone in this way. Of course, in this plate they account for that by the direction of the strain, but I am alluding more to *ship's* plates. They also have always been found to have cracked in that unaccountable manner *across* the plate. One instance came under my notice of a steel plate, an inch thick, which had not any work on it, being part of the engine seating of a vessel. That, while being riveted in its place, cracked

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across in the way that this one is indicated to have done. On enquiring into that matter and conferring with the makers of the steel, going down there, watching the rolling of the plates, and trying some experiments, we found it clearly to be the case that if a plate was stopped at all in the rolls it was liable to the result that has been given here; and I would, therefore, suggest whether it is not as likely or more likely, that the cause of this fracture was neither in the original ingot nor in the heating of the plate in the boiler-maker's furnaces, where, although the heat might be irregular, still it would be general, and the change of temperature in the different parts of the plate would be quite gradual, whereas in any stoppage in the rolls, especially at the commencement of the rolling, if the rolls were pretty cold, there is upon one clean line across the plate that sudden change of temperature which, as I suggest, might be probably the cause of such an unfortunate occurrence as the one we have had placed before us.

Mr. B. Martell: My Lord, I feel it is rather presumptuous on my part to stand up here to discuss a Paper of this kind, but I much fear that this very important Paper is going to pass without that full discussion which it has been suggested should be held by those who are most competent to hold it. I am very much surprised to find that so few steel manufacturers and boiler makers are here to discuss this very important Paper. I cannot help thinking myself that it is one of the most important Papers that we have had brought before us in this Institution for a very long time. When we are increasing the pressure of steam boilers to the enormous extent we are doing at the present time, and find that in order to do that we must have these thick plates which are now being made, such an accident as this I think is a matter that calls for the most serious consideration of every one connected with marine engineering as regards extending these excessive pressures. With reference to the remarks that have been made and I am very sorry to say so, with all due deference to every gentleman who has spoken, it seems to me we are really going away from the main point. Mr. Kirk says, what have we to do with carbon? Then I would ask in reply, what do we want laboratories for? It has only been recently suggested to us that we can do away with mechanical tests altogether, and that all that is wanted is to send samples of every cast to the laboratory, and let the analyst ascertain what amount of constituents there are in the material, and that would be perfectly satisfactory without further tests. It is only a little time ago that I was on the Continent surveying some steel ships where the plates had failed, and a very eminent steel manufacturer came into my office a few days after my return, and I showed him the analyses of these plates—plates which had cracked unexpectedly. He said "Well, I am not surprised to hear from you that such is the case. The carbon is far too high; I am not at all surprised at the material failing." This was a very eminent man, whom, if I were to mention his name you would know. He added "I do not think you need go any further into the matter." Then we find men who come from the School of Mines, who study these questions, and who are able to tell you how to produce material which never could have been brought into existence for us if it had not been for their chemical knowledge and competency to analyse the component parts, and tell us the exact proportions necessary to secure good steel; and yet we are told by an eminent engineer here that the question of carbon is subsidiary and a matter of no moment. What I want to know is this, whether you can allow a proportion of carbon of ·33 as found in this plate, and produce the same of good ductile material? No steel manufacturer has told us, he has not entered upon that question at all, he has not explained that to us, and that is the crucial thing we want information upon. The scientific chemists who are

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attached to all these great steel works can tell what kind of material they are going to produce. We know very well that when steel ingots are produced they test them, and if they are a little too high in carbon they say, put that on one side that will do for rails or something else—put this on one side, it will do for something else; and they know precisely the amounts of the various constituents in that steel that will cause it to be satisfactory for certain purposes as regards its ductility and strength. Cannot the steel manufacturers here tell us whether they can get plates 1¼ ins. thick having a proportion of ·33 per cent. of carbon to fulfil the required conditions as regards ductility and extension to the same extent as they are found to answer with regard to thinner plates for shipbuilding purposes? I believe myself that steel manufacturers know all about it if they would come here and tell us, and I will give my experience of that. Mr. Denny knows very well, in one ship he was building, there were some steel plates that failed. I went down purposely to examine those plates, they were thick plates, because it was a large ship.

Mr. Denny: Mr. Martell, they were plates about ½ in.

Mr. Martell: Some of those plates were more than ½ in. thick.

Mr. Denny: They were not thick plates in that sense.

Mr. Martell: It is not a question of the thickness of plates, but I was merely going to illustrate my observation, that if steel makers were as anxious to come forward and give us information as we are anxious to receive it, there would be no difficulty in their doing it. For instance, I said when I went to examine those plates, it is very important to have the manufacturer here. I wired him, but he would not come. I said I was not going to leave till he came. Mr. Denny then used his influence and got him down, and I pressed him very hard, and eventually got out of him the cause of the failure and he wrote to me saying similar failures would not occur again. I believe with reference to the constituents of this material that if the steel makers were to come forward they could tell us all about it. It is not in this case a question, as Mr. Raylton Dixon observed, as to the passing through the rolls, but here you have the material which Mr. Parker tells you was put across an anvil and which with one blow broke suddenly off, and showing by analysis an unusually large percentage of carbon. Does the mere difference in stress which the boiler maker might have put on it in that material alter the chemical analysis of it? Certainly not. There might be a mystery about it if this steel had broken, and, upon chemical analysis, was found to have only the same constituents as plates that were satisfactory. But it has been said by several who have analysed it that it is quite a different material from ordinary mild steel, and I know between ourselves that many of them would say it was totally unfit for marine boilers.

Mr. A. E. Seaton: My Lord, I rise in answer to a challenge of Mr. Martell as to boiler makers being present and not speaking. Some few years ago there was a trial trip, during which the engine-room clock was discovered to have stopped. There ensued immediately a very strong discussion among the learned men on board as to whether it was caused by the heat of the engine-room or the vibration of the engine. One party took one side, and another party took another, and both got to very high words, when suddenly a small boy informed them that the clock had not been wound up. We have been talking about the rolling of the steel, and its subsequent reheating in a furnace, but I think there is one point we have all overlooked. From the position of the fracture in the boiler shell, I have been led to believe it is very close to the landing of the front plates, and probably the top plate immediately above the tube

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plate is jointed to the tube plate very nearly at the point shown by the fracture. I should like to ask Mr. Parker—or perhaps Mr. Boyd could answer it better—whether he is quite sure the boiler end plates were properly closed? What leads me to ask this question is the fact that Mr. Boyd significantly pointed out that that end was a hand-riveted joint. On looking at the boiler end it struck me that that was evidently close to the point at which the top plate and the tube plate met. I would ask Mr. Boyd if he is quite sure that such a thing as a drift was not used there, or that the boiler makers did not put a heater or build a fire at the place in order to close the plates. I do not say that that is the cause; but, as a boiler maker, I would like to find out the cause of this boiler breaking, and I know that such things are done occasionally. I find it a most difficult thing to drum into the heads of both the foremen and workmen the danger of working at the blue-black heat. I have lent my foreman boiler maker books and papers, but he only expresses grave doubts about it, and thinks the blue-black heat is a nice subject on which to read papers, but one that does not apply, according to his ideas, to the work in the boiler shop. I do not say I am sceptical of the blue-black heat theory, because I am not; but I think it is often a convenient scapegoat to account for breakages. At the time of the *Livadia's* disaster, we were using a large quantity of Cammell's steel, and there was not in that steel the faintest indication of what was found in the *Livadia's* steel. For the last two or three years we have been using a great deal of Messrs. Colville's steel, and although there is a gentleman from the Steel Company of Scotland present, I do not hesitate to say that Messrs. Colville's steel is quite equal to that of the Steel Company of Scotland. I do not think there need be any doubt thrown on the quality of Messrs. Colville's steel, because I have never seen nor used better steel than theirs, and I have tried steel from almost every steel maker in the kingdom. I think that what Mr. Denny said was very pertinent, and that, having got to a tensile strength of 32 tons in the construction of a boiler, we should not make a retrograde movement. I say this advisedly, because we know, as a matter of fact, that you can get from Sheffield steel castings, whose ultimate tensile strength is 34 or 35 tons, with an elongation very considerably more than Mr. Parker has given this evening. I think my experiments, which were published last year, in the transactions show that. I say, if this can be got from a steel casting, why cannot it be got from a steel plate? I trust that steel makers will not be frightened and driven back by what has occurred now. No doubt it is a very serious disaster for a boiler to burst under test, but I think it should only lead the steel manufacturers to try and get—and I am sure they can do it—a reliable steel with a tensile strength of 32 tons, or even 40 tons, for the matter of that. There are one or two points on which I might have touched, but, as it is so late, I will say no more now; but I cannot sit down without apologising for having spoken at this late hour. My only excuse is that the subject is one demanding serious investigation, and I think it points to the fact that this Institution would do well if it occasionally had the whole evening for the discussion of a single Paper.

Mr. H. H. West: My Lord, I notice that the hour is late, but, as the society which I have the honour of serving has been mentioned, I think I may be excused for making a few remarks. In the first place, I would echo most cordially what Mr. Denny said in thanking Mr. Parker for the great pains which he has taken in giving us all the facts of this case. In the next place, I would like to say that the remarks made by Mr. Kirk, by Mr. Denny, and by other speakers had not exactly the tendency that Mr. Martell seems to imply. It seems to me that if anybody wanted to wrap this discussion up in a nutshell and dispose of it, it was Mr. Martell who was the sinner, and not those who preceded him, all of whom said,

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Do not let us come to a conclusion in a hurry, but let us consider the subject calmly and dispassionately. I, myself, would like to emphasise that, because I think that the tendency of Mr. Martell's remarks was to dispose of this purely and simply upon one element of the question—the excessive amount of carbon.

Mr. MARTELL: No, no; certainly not.

Mr. WEST: I thought it was.

Mr. MARTELL: Certainly not.

Mr. WEST: Then I beg your pardon. It appears to me, my Lord, that in Mr. Parker's analysis of this case we have a feature that I think has not been commented upon hitherto, and that is this, that several eminent metallurgists on analysing the plate gave widely different results. Now, I presume they gave different results because their samples were taken from different parts of the plate; but I do not see how Mr. Parker can connect the fact that different parts of the plate had different chemical constituents, with the tension limit of the steel being high. At the commencement of the Paper the material is spoken of as being 29·6 tons tension. Now, I think that this is rather a remarkable fact, because Mr. Parker says that the tenacity of thick steel boiler plates should not exceed 30 tons. In this particular plate the tenacity did not exceed 30 tons, in fact it was below 30 tons. I think that suggests, as Mr. Kirk has said, that it is not the tension that is at fault at all, but some other characteristic of the material, which we may or may not have ascertained, but which is certainly suggested by the irregular results of both the chemical and the mechanical tests. In reference to high-tension steel, the Underwriters' Registry have had under their inspection an immense quantity of plates of all sorts of high tension, and I may say that as a rule, if the samples have failed in the bending tests, they are found to be of rather low tension than high tension. I do not mean to say there are no failures in the high-tension plates; I should be sorry to make such an assertion, but, at the same time, I must say there are not more in the high than in the low-tension material—possibly, if anything, fewer. For myself, I feel assured, from the quantity of high-tension steel we have been successfully using for some years past, that we have no need to fear steel of a tenacity of 30 tons, and even upwards. If you get steel which is of a much higher tension than 30 tons, your suspicions may possibly be aroused; but, if so, then pursue your investigations into the other qualities of the steel as closely as you like, but do not condemn it for being too strong. I would like to notice a remark of Mr. Parker's last night—I did not take down the exact words, but I was very much struck with them. He said that until we got steel we were confined to boiler pressures of 70 and 80 lbs., and I should have thought from this that Mr. Parker would have been the first to ask us not to go back on the tension limits of our steel, but to go forward, and I think if we can succeed in keeping where we are (and I see no reason why we should not) we have at any rate made good our step, and I think a further step should be in the direction, not of reducing the tension, but of increasing it.

Mr. A. C. KIRK: My Lord, I know it is against the rule, but it is only about two words I have to say.

The PRESIDENT: They must be strictly in explanation.

Mr. KIRK: It is explanation only. The strength Lloyd's impose for *all* the steel of a boiler is 26 to 30 tons, but, taking a very sensible view of the case, they reduced the factor of safety in thick plates.

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The Board of Trade again, in an enlightened manner, recognised the difference between shells and furnaces, and for the former permit steel of from 28 to 32 tons. The result is that the plates are practically of the same thickness on either system. As to thick plates, I understand the Steel Company of Scotland are making most admirable steel, and thick, too, for the great Forth Bridge of a high tensile strength, I think of 33 tons per square inch.

Mr. Boyd: My Lord, shall I be in order in answering one or two questions?

The President: Only if it is in the way of explanation.

Mr. W. Boyd: Very well, my Lord. I wish to answer Mr. Seaton's question primarily. Mr. Seaton asked in a pointed way a very pertinent question, as to the way in which this front plate had been put in and connected with the rivets, which I stated at the opening of the discussion were put in by hand. I need not say that this point received our attention at the very outset when we cut the defective plate out of its place. The front plate is put in in the usual way. Those who are accustomed to working in a boiler shop will know that it is not the easiest thing in the world to get an end plate into the shell of a boiler, and that it must be forced in with screws and drifts, and other extra and outside appliances, simply for this reason that it has to be forced into the cylindrical shell. All I can say in answer to Mr. Seaton's question is that the ordinary process was used which has been used, I think Mr. Parker said, in the 175 steel boilers that we have made. With regard to his other question, as to whether any heater had been used, I am able to say most distinctly that there was nothing of the sort, and that the end plate was screwed into its place in the usual way. Then reference has been made to the furnaces, of which unfortunately the sketch has been rubbed out, because a good deal seems to hinge upon that. I would simply like to say this, that the shape of that furnace, as I think I stated before, is the usual type—there is nothing abnormal about it. We have heated ourselves something like 1,400 plates in it, and this plate was treated in exactly the same way as its predecessors, and I desire that it should be clearly understood that the furnace being 21 ft. long inside, clear of the doors and flues, the plate being 20 ft. 7 ins. long was entirely contained in the furnace, and that no part of it projected outside thereof.

Mr. J. H. Heck: My Lord, I had intended not to speak on this subject, but I should like to say that I made some experiments last year, in the performance of my duty, on a number of steel plates, and one result I noticed more than anything else was this, that whenever a sample broke simply by the mechanical bending test it was nearly always a sample of high tenacity. I may say we tested some thousands of plates, and whenever a sample broke we were always careful to re-test it, and we invariably found that the samples that broke in the bending test were samples of high tenacity. It is quite natural to assume that a thicker plate should be of low tenacity if you want to do much bending upon it. The plate in question has a tensile strength of 34 tons, and I have seen many thin plates of a similar tenacity bent double without fracture; but with thick plates the tenacity should be lowered if you want to do the same, or a greater amount of work put upon it; there is no question about that in my mind. Nearly all the cracked steel plates I have seen were thick plates. I put that forward as I have seen a great number of tests made. I do not, however, wish to say anything against

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high tenacity because I agree for some purposes, notably for shipbuilding, it is suitable within certain limits. That is my opinion upon the point, but for boiler making where you have to use plates 1¼ in. thick that have to be bent and worked, I think that plates of such high tenacity cannot be used with safety.

Mr. J. F. Hall, of William Jessop and Sons, Limited, Sheffield: My Lord and Gentlemen, as a steel manufacturer, after what Mr. Martell and others have said, it is impossible for me to sit here without making a few remarks. I think, perhaps, I might say something first relative to the analysis. If Mr. Martell, as one of Lloyd's chief surveyors, is going to run his head against the analysis of metals he will soon find himself buried in the depths of a mystery that will take him some time to get out of, and meanwhile he will in all probability, without knowing it, do some innocent steel manufacturer a gross injustice, which I know would be against his feelings. Steel manufacturers themselves barely know all the mysteries of analysis, and if surveyors are going to dabble with it, I for one shall most strongly object. I speak on this Paper with very great deference, because I feel that in doing so I run a certain amount of risk of doing injury to some person unknown. The data we have to go upon is very meagre indeed to give a decision as to the cause of the fracture. From the few facts I have learned from this Paper it certainly appears to me that the steel manufacturer is not absolutely to blame. To say that that plate would have fractured in the way it did, because of the difference in carbon varying from ·36 per cent. to ·26 per cent. or because it had the high tensile strain of 33·1 tons, or because it only had 21 per cent. of elongation, is to my mind really too absurd. Such a fracture as that could never have occurred in that plate had it been properly treated. Mr. Dick has really hit the right nail on the head when he said that the steel had been spoiled in manipulation afterwards. This plate, as I understand it, was a very long one, and it was re-heated in a furnace that was too short for it. It had to be turned round and put in the other way again. Well, if that was so it is perfectly plain that the plate was not properly heated. The best steel in the world, either soft or hard, can be spoiled by fire, and it is very evident to me that this steel has been spoiled by fire. It may have been too much fire or too little fire, but it was fire. If this plate had been bent at the blue-black heat which has been spoken of by Mr. Dick, and had been left in a state just ready for breaking, that is one theory, and I think a very likely one to have caused this fracture. Another thing that might have occurred is that the steel might have been overburnt. Steel can be burnt in the same way as wrought iron. Wrought iron is often greatly deteriorated by being overburnt, and so is steel. That is another reason; but without all the facts before us, it is simply impossible for us to come to any definite conclusion as to the cause of this unfortunate accident. It is an unfortunate accident in more ways than one. If it had been an iron plate nothing would have been said about it, but because it is a steel plate there is all this row about it. This being a time when steel is undergoing such an acute examination all round, I think it is particularly a misfortune that this plate should have cracked in the way it has done. I cannot, however, as a disinterested steel manufacturer remain in this room without saying that from the facts which have been brought before us, I, for my part, do not think that the steel makers are to blame at all for the fracture, although the quality of the steel, as shown by the analysis, certainly might have been better.

Mr. W. Parker: My Lord and Gentlemen, I must thank you for the extended discussion, and the privilege you have given me to-night in allowing this Paper to be read and discussed.

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I have listened to what has been said with great interest. I must own I have been somewhat disappointed at the expressions that have fallen from the steel manufacturers, but at the same time I am not sorry.

When this accident occurred, and I was instructed to investigate it, I considered it my duty to confer with the steel makers especially, and with everybody else concerned, with a view to clearing the matter up. I sent to the most eminent steel makers in the country, including those that have spoken to-night, pieces of the material, not pieces of the plate, such as have been referred to by some of the speakers, but shavings taken from the edge of the plate in the following way: a piece of the plate was put into a planing machine, and shavings were taken from it which were afterwards mixed together and put into small boxes, one box being sent to each steel maker, so that it was quite impossible for them to know what the material was part of.

I asked the steel makers to be kind enough to analyse the material and send me the results. This was done, and not only did they send me the analysis, but in nearly every case, including those who have spoken to-night, they distinctly gave it as their opinion that this steel was altogether unfit for boiler-making purposes, even to the extent of being unsafe.

For this reason I feel somewhat disappointed, as I said before, at what has been said by the steel makers after having expressed totally different views to me, both when at their works and in writing.

Before going further, I would just like to say that I wish it to be clearly understood that nothing I have said, or may say, is intended to reflect for one moment upon either the makers of the steel or the builders of the boiler. I believe the steel to have been good, but I also believe that it was not suitable for the purpose for which it was used; and although it was steel such as we have been passing daily for such purposes, yet I contend that this investigation shows it to have possessed a quality which rendered it brittle when subjected to ordinary usage in the boiler works.

In answer to Mr. Boyd, I may say with regard to the temperature of the plate when rolled, that the plate I saw rolled was quite cold at one end, so much so that I was able to place my hand upon it when it was being finished, while the other end was at a blue heat.

When my investigations were drawing to a close I considered it right to write to Mr. Boyd's firm and also to the steel makers, to ask what observations they had to make. Mr. Boyd's answer was to the effect that so far as the cause of the accident was concerned, he was unable to give any explanation, and if he could he would have done so long before. On the part of the steel makers, the reply was "The only conclusion we come to is that the plate was improperly manipulated."

Mr. Cuthill has given an exhaustive description of how he considers the accident to have occurred, viz., by unequally heating and badly manipulating the plate, and he concludes his remarks by saying that he considers steel of that nature to be perfectly safe. All I can say is that that was not what I understood to be his opinion when I saw him at the Glasgow works.

Mr. Denny says that one swallow does not make a summer, and seems to rest perfectly satisfied in the knowledge of the fact that the hydraulic test found out the brittle plate.

I am quite willing to agree with him that it would be a very great pity indeed for us to get into a panic merely because a single plate has cracked. But I should like to remind Mr. Denny that the hydraulic test which is required to be applied to steam boilers is not a test for the purpose of proving the strength of the boiler at all, but simply a test of workmanship. These boilers are supposed to have a

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factor of safety of from five to six times their working pressure; that is to say, this boiler should have withstood a pressure of from 950 to 1,000 lbs. per square inch before it gave way, and yet, when it gives way at 240 lbs., it is considered by some of the speakers to be of little importance. It is true, as Mr. Denny suggests, that one swallow does not make a summer, but I cannot say that I look upon this case so lightly.

Mr. Macfarlane Gray asked if I had ascertained whether the plate at the fracture was hard or not. I did ascertain that, and here are the samples. Here is the fractured plate; this is the fracture itself; that sample was cut from the plate at the crack, so that it would give the condition of the material at the time of the accident. As I say in my Paper, it had a tenacity of from 33·5 to 34·2 tons per square inch, and an extension of 16 per cent., as compared to a tenacity of 29·5 tons, with an elongation of from 22 to 28 per cent. before it was put into that boiler, clearly proving that the material had altered its condition, and hardened and become brittle. Mr. Kirk is somewhat surprised to think we should call this an alarming accident, and he has referred to plates which have cracked on previous occasions. That subject has been discussed here over and over again. I think when Mr. Kirk read his Paper on the question it was well discussed, and although Mr. Kirk was not here to reply to the criticisms, still, to my mind, some of them pointed out what strains broke the plates, and how those strains were set up; especially those of the late Sir William Siemens and Mr. Thornycroft.* The *Livadia's* boiler shells were the first that caused any alarm, and in their case I have no hesitation whatever in saying that the shell plates were bad steel. I had the privilege of investigating that case, and I think the Paper I read on the subject before this Institution to some extent cleared that matter up. The plates that have cracked from that time to the present have all been thin flanged plates, intended for internal parts or fronts of boilers, and they have, in my opinion, cracked or torn because strains have been set up in them through unequal heating in the operation of flanging. There have been nearly 6,000 steel boilers made for steam ships since the *Livadia* accident in 1881, and during that time we have not had a single case of a shell plate giving way. I wish distinctly to draw attention to the difference, in my opinion, between the tearing or cracking of thin plates which are subjected to local heating and flanging, and the bursting of a boiler shell, such as is the subject of this Paper. Mr. Gray made a suggestion with regard to the cracking of thin plates, and the strains set up in them by unequal heating, and to show that I agree with Mr. Gray on this subject, I will describe an experiment which I had made to try and tear a plate by local heating. Mr. Dick, of the Steel Company of Scotland, was kind enough to make me a large plate about 12 feet long and 4 or 5 feet wide, and for a distance of about 2 feet 6 inches the corner of the plate was heated to a white or working heat. At the back of the line of heat was placed a wall of green coal and sand, and a brick wall was built against that. A stream of water was then played on the plate, so that the back part of the plate should be in as cold a condition as possible, compared with the portion in the fire. It was expected that when the plate cooled the unequal contraction would be so great as to tear the plate from the edge; our anticipations, however, in this instance were not realised, and from want of time I was obliged to desist from further trials in the same direction.

Mr. Macfarlane Gray: That was not my illustration at all; there is no bending of the plate there.

* See Volume xxiii. of *Transactions*.

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Mr. Parker: I beg your pardon, I thought it was.

I will not trouble you further, except to say that in placing before you, this evening, the results of my investigation in this matter, I have been actuated by the thought that, seeing the prominent position which this Institution and the Society which I represent have occupied in the introduction of steel in the construction of marine boilers, and in the promotion of higher pressures, it was incumbent upon me to lay before this Meeting the facts of this case, and to show, that, in my opinion, the accident which has happened clearly warns us that we have been unknowingly drifting into the use of an unreliable material, and that it is now high time for us to pause awhile in our forward movement, and consider seriously how far we ought to go with thick steel plates of high tenacity.

In conclusion, I would like to put a question to both steel makers and steel users; firstly, do steel makers really believe that steel plates having a tenacity of 32 tons per square inch, and of such a thickness that the material must contain from ·25 to ·3 per cent. of carbon, are reliable, when subjected to the ordinary work that goes on in a boiler shop? and secondly, do steel users consider a material that will take a temper and, under the ordinary work that goes on in a boiler shop, harden, is a fit and proper material for large marine boiler shells. My own reply, to each of these questions, is decidedly, No. However, the whole of the facts have now been brought under your notice, and once again I venture to impress upon all interested in the question, the absolute necessity of giving the subject the consideration which its importance deserves; and having done that, I have finished.

The President: Gentlemen, I am quite sure you will allow me to return Mr. Parker our thanks for this very interesting Paper. I am sure he has been actuated by a sense of public duty. I hope he is satisfied with the discussion which has arisen upon it, and I congratulate you all on the perfect temper and calmness with which the discussion has been carried on. I am very glad that has been so, and I am quite sure you will not regret the lengthened discussion which has taken place upon this most important matter, and which has created so much public interest out of doors.

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THE PROPELLING MACHINERY OF HIGH SPEED SHIPS.

By E. A. Linnington, Esq., Member.

[Read at the Twenty-sixth Session of the Institution of Naval Architects, March 27th, 1885; the Right Hon. the Earl of Ravensworth, President, in the Chair.]

In the interesting class of war vessel known as Cruisers, and in many large ships of the mercantile marine, speeds have recently been attained much in excess of those which prevailed some years ago, and it is highly probable the present high standard of speed will be maintained, if not surpassed, in future ships. The present moment, therefore, is not inopportune for a few remarks upon the propelling machinery of these high speed ships.

In Cruisers, the proportion of I.H.P. to displacement varies from $1\frac{3}{4}$ to $2\frac{1}{4}$, according to the size and speed. This fact alone shows that any reduction of the demands made by the machinery upon the displacement and space is of the utmost importance. Further, it is considered that, as the maximum speed will only be required on special occasions, economy of fuel at the full power is not the most important object to be obtained. The direction in which engineers are proceeding to meet these conditions is to force the boilers with the stokeholds under air pressure, to use steam of about 120 lbs., with a low rate of expansion so as to secure a large proportion of mean to initial pressure, and to run the engines at a great piston speed with a high number of revolutions. Experience proves that with these methods it is quite practicable to produce a very compact arrangement of sea-going machinery, with twin screws and duplicated engines, on a weight not exceeding 2 cwts. per I.H.P., this weight including the water in the boilers and condensers, the spare gear, and fittings usual in ships of war.

The method of forcing the boilers was fully described and illustrated before this Institution two years ago by Mr. Butler, who gave the particulars of the trials of H.M. Ships *Satellite* and *Conqueror;* and further experience has served to strengthen the good opinion formed from those early trials. The boilers of the above ships were not, in the first place, intended to work with forced draught, and the proportion of heating surface to grate surface was that usual for natural draught. In the boilers now under construction

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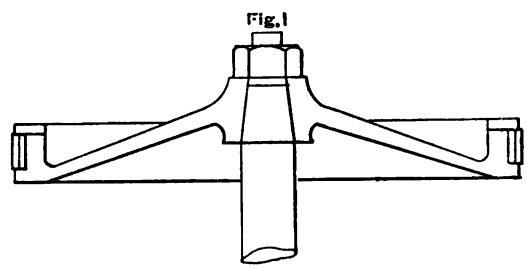
the proportion of heating surface has been increased so as to secure greater efficiency when forcing. With the low boiler this extra heating surface has been provided by simply adding to the length of the tubes, and as the temperature in the smoke-box of the *Satellite* was about 1,100° Fahr., it is evident the additional length must add largely to the efficiency.

Very high piston speeds have recently been obtained, the highest in H.M. Twin-Screw Cruiser *Phaeton* 5,500 I.H.P., the engines on trial running at 800 feet per minute, the revolutions being 100, and the stroke 4 feet. The engines of a large number of Cruisers now building are intended to work with a piston speed of 750 to 820 ft. per minute at the full power. These are all horizontal, two-cylinder compound engines, with the cranks at right angles. The proportion of mean to initial pressure is very large, and this gives an engine well adapted to quick running, for the small cylinders have small reciprocating parts for the work they perform, and the late cut off tends to uniformity of the twisting moments on the crank shaft. Light reciprocating parts and uniform twisting moments are two most desirable features in a high speed engine. The efficiency of the steam is low at the full power, which, however, is seldom required, but at low powers these engines are very economical, not only on account of the efficiency of the steam, but also on account of the small initial friction which attends the use of small pistons, piston-rods, and shafts. It is well known that when engines of large capacity for the maximum I.H.P. are run at low powers, the greater part of the power disappears in overcoming the initial friction.

The cylinder liners, pistons, piston rods, connecting rods, and shafting are made of steel, a material which, compared with iron, possesses qualities favourable to quick running, for the greater hardness and smoothness of surface much reduces the wear and tear, and the superior strength lightens the reciprocating parts. Steel cylinder liners have been used in the Navy for several years; a cracked liner is a rare curiosity, and the wear is taken by the softer spring ring of the piston. The pistons are of cast steel, of the form shown in the sketch (Fig. 1); the spring rings are usually of cast-iron. With this dished shape stiffness is the first consideration, and with proportions which give a weight about 35 per cent. less than the weight of a cast-iron piston, the deflection under a water test of twice the working pressure is found to be inappreciable. Steel liners and

Fig. 1



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these light pistons permit the highest piston speeds in horizontal engines of moderate dimensions, without the use of tail rods or back guides.

With twin screws in a suitable position, the fineness of the run of high speed ships necessitates a great length of shafting outside the ship, and the distance between the stern tube and stern bracket is sometimes too great for the shaft to run unsupported if made of the ordinary diameter. Intermediate bearings have been used, but the most recent practice is to use a hollow steel shaft of enlarged diameter between the bearings, as shown in the sketch (Fig. 2), which illustrates the arrangement in the *Mersey* class of Cruisers,

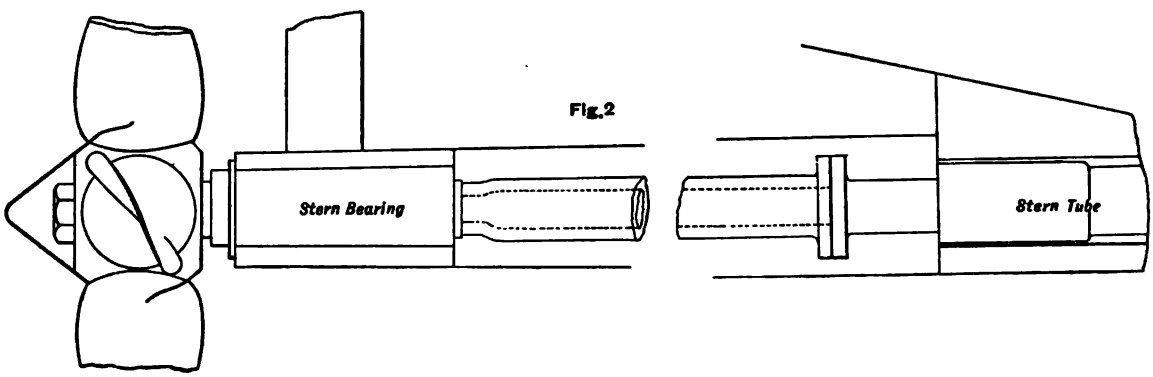

Fig. 2

where the length between the stern bracket and stern tube is nearly 45 feet. This, simple as it appears, must be regarded as a great improvement, as the intermediate inaccessible bearing is dispensed with, and its bracket or other support is not required. The removal of the bracket effects a considerable reduction of the resistance due to the hull appendages of the twin screws.

A few years ago, the proportion of I.H.P. to displacement in the swiftest of the large ships of the mercantile marine did not exceed $\frac{6}{10}$, but in some recent ships, the horse power equals the displacement, and any reduction of the weight and space required for the machinery has, therefore, assumed some importance. Economy of fuel, however, is the first consideration, thus even in ships of the greatest power and speed the boilers are worked with natural draught, and the only reductions of capacity have been those permitted by the use of higher pressures of steam and higher rates of expansion. The gains in economy, and the resulting reductions of boiler capacity, which attend equal increments of pressure, become less and less as the absolute pressure increases, while increments of pressure mean increments of the thicknesses of plates. Sustained efforts may be made to reduce the capacity per horse power of the boilers, and to improve the efficiency. Unfortunately, the method of forcing

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which is so advantageous in war ships, is not likely to be of much service in merchant ships for reducing capacity, for the ordinary marine boiler is not well adapted for providing in a small capacity the proportion of heating surface to grate surface necessary for the economical use of forced draught, and a small saving in the size and first cost of the boilers would not compensate for the deterioration which must inevitably accompany *continuous* forcing. Experiment is wanted to determine the possible efficiency of marine boilers under forced draught.

Unusually high piston speeds have been run in the engines of these ships, and the increase has been obtained by raising the number of revolutions. The increased speed has kept down the size of the cylinders, and the increase of the revolutions appears to have been necessary owing to the limited screw discs upon which the great powers have been employed. Many instances of speeds over 700 feet per minute could be cited, but only two are given. The *Oregon's* engines have developed at sea 13,000 I.H.P., with a piston speed of 768 ft. per minute, revolutions 64, stroke 6 ft. In the *Umbria*, on a measured mile light draught trial, with 14,300 I.H.P., the revolutions were 70, and the piston speed was 840 ft. per minute. The three-cylinder engine has been generally used for these high speeds. This type has a tolerably uniform twisting moment, and no great accumulation of reciprocating parts over one crank.

Permit me now to request your attention to a few observations upon the use of twin screws in the high speed deep draught ships of the mercantile marine.

In 1878, Mr. W. H. White read a paper before this Institution, in which he considered the disadvantages and advantages attending the use of twin screws in merchant ships, and only a brief reference will now be made to them.

The twin screws being much exposed, are liable to injury when docking, &c. To meet this difficulty, the stern may be widened so as to effectually screen the screws, or, as in H.M.S. *Polyphemus*, a portable above-water protector may be used.

The two sets of engines take up more space between decks than the single set, and the great width of hatch may prohibit the usual arrangement of cabins, &c., over the whole length of the engine room. The slide valves, however, may be arranged on the sides of the cylinders towards the middle line, so as to shorten the engine room, and reduce the width of hatch as much as possible.

The first cost and the working expenses will be greater with the duplicated machinery.

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Against the above objections may be set the surpassing advantages of freedom from total loss of the propelling and manœuvring powers of the ship; for the probability of the two sets of machinery breaking down at the same time is a very slight one, and as twin screws will steer the ship without the aid of the rudder, they may be said to duplicate the steering apparatus. The chances of a break down have been lessened by the introduction of steel shafting and steel or bronze screw blades, but in this special class of ship the advantages of the twin screws will probably be considered to balance the objections enumerated, and yet twin screws have not even had a trial in one of the high-speed merchant ships.

We must then seek another objection, which is probably found in the opinion that the use of twin screws would be attended by a loss of propulsive efficiency, which means that for the production of an assigned speed a greater I.H.P. would be required with the twin screws than with the single screw, and that the objections already noticed would be intensified, and the great disadvantage added of larger boilers and greater consumption of fuel.

In the paper of Mr. White, to which reference has been made, it was shown from the results of measured mile trials, that in a group of deep draught ironclads the mean gain in propulsive efficiency with the twin screws was approximately 18 per cent.* In passing from these ironclads to the merchant ship we go from a full form to a much finer form, from a speed of 14½ knots to a speed of about 18 knots, and from two-bladed screws of the original Griffiths' shape to four-bladed screws of a fuller form towards the tips of the blades. These differences may affect the relative augments of resistance, the gains from the wake in the efficiency of the screws, and the lengths of the shaft tubes and brackets; but 18 per cent. is a large margin to draw upon, and the assumption that the same I.H.P. will propel the ship at a given speed with either single screw or twin screws does not appear unreasonable.

Any attempt to compare the relative propulsive efficiencies even in the same class of ship, except by actual trial, finds us in a vast sea of conjecture. The effective horse power (E.H.P.), or the horse power to overcome the resistance of the naked hull is the same with either system, but the following items may differ considerably and modify the propulsive co-efficient $\frac{\text{E.H.P.}}{\text{I.H.P.}}$

* The mean gain for two groups of ironclads was 15 per cent. Twenty years ago Messrs. Dudgeon had fitted a number of blockade runners with twin screws, and were able to say "We fearlessly claim an advantage of at least 15 per cent. over any known method of applying power to a steamer."

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We have to consider :—

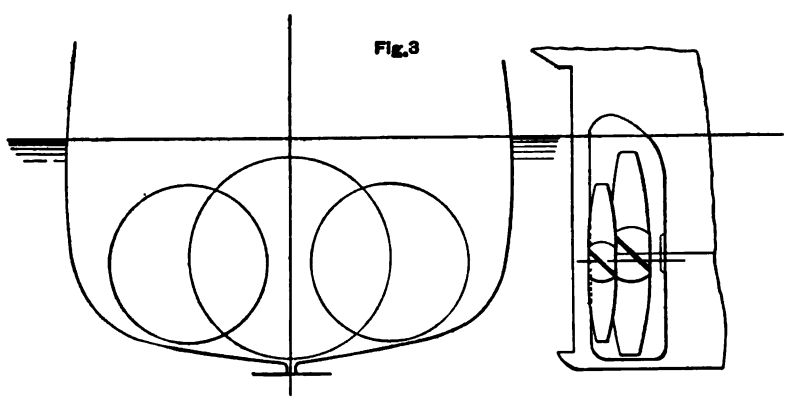
Augment of Resistance
(1.) Augment of resistance due to the action of the screw or screws.
(2.) Augment of resistance due to the rudder post with the single screw.
(3.) Augment of resistance due to the shaft tubes and brackets with the twin screws.

Efficiency of screws.
(4.) Screw efficiency in undisturbed water.
(5.) Gain from wake in screw efficiency.

Efficiency of engines.
(6.) Initial friction of the engines and shafting.
(7.) Working load friction of the engines and thrust blocks.
(8.) Loss from clearance and condensation.

The augments of resistance due to the twin screws, and also the efficiency of the twin screws, must be dependent upon the positions assigned to them, and the positions may be varied considerably—transversely, vertically, and longitudinally. Speaking generally, the nearer the screws are to the hull, the greater is the augment of resistance, and the greater the interference of the hull with a good flow of water to the screws. Fig. 3 shows positions which experience has proved to be satisfactory.

With the relative positions of the screws in Fig. 3, the augment due to the action



of the twin screws must be much less than the single screw augment, the larger part of the twin discs being remote from the hull, while the fullest horizontal sections of the run are immediately in front of the upper part of the single screw disc, the locality of a large slip much exceeding the mean slip. Ships of fine form have less augment than the fuller forms, and the twin screws may obtain less advantage compared with the single screw in finely formed high-speed ships than in ships of lower speeds and fuller forms.

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We may say that the twin screws gain considerably with (1), but (3) will generally be greater than (2), and for similar engines the losses per I.H.P. from (6), (7), (8) will, as a rule, be greater with the twin engines than the single engine. The single screw may be expected to have the advantage in (5), but no general rule can be given for the relative efficiencies of the screws.

The values of the gain from wake (5) and the augment of resistance (1) have probably been submitted to experiment with model screws set behind model ships. Now the great frictional resistance of the short surfaces of a model gives to the wake an undue forward momentum, which may cause the single screw to indicate an advantage compared with the twin screws not attainable in the full size ship.

If the single screw be immersed, and of an efficient diameter, there appears little reason to expect a great gain in propulsive efficiency by the substitution of twin screws for single screws in high speed merchant ships, and without trial in the full size ship, it cannot be said there would be any loss of efficiency.

With any further advance in horse powers and speeds, the above conditions of single screw efficiency will probably not exist, for the draught of water on the Transatlantic service is at present limited to a little more than 26 feet, and screws over 24 feet in diameter have been used in the most recent ships. Any increase of screw disc would lead to some loss of efficiency, owing to the constant emersion of the tips of the blades, the average loss on a long voyage exceeding the loss on a load draught trial in smooth water. Moreover, any increase of speed can only be obtained by increasing the revolutions, for there is an absence of practical experience in favour of larger pitch ratios than those lately used, and unless future ships are of much finer form than the most recent ships, no reduction of blade area can conveniently be made. The proportion of blade area to disc area is already very large, and any increase of revolutions may cause some loss of efficiency, owing to the blades acting upon water disturbed by their immediate predecessors; such disturbance increasing the slip and loss due to slip without a compensating diminution of the blade friction. Under these circumstances, the propulsive efficiency with the single screw would probably compare unfavourably with that with the twin screws.

If any great increase of speed and power be attempted, the twin screws may be used with some assurance of no loss in propulsive efficiency, and the greater the horse power the greater the necessity for a duplication of the machinery. A twin-screw ship, with

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large passenger accommodation, could scarcely prove, under any circumstances, a commercial failure, for if the virtues of the twins and the evils of a break down in mid ocean were duly set forth, the ship would receive extensive patronage from the travelling public.

To secure the advantages of duplicated engines in all cases of emergency, each set of engines should be placed in a separate watertight compartment. This could be conveniently effected by a bulkhead on the middle line, with a watertight door at the platform or other level.

One cannot leave this interesting subject without the somewhat trite remark that the large high speed ships of the mercantile marine would be of great service to the State in the event of a maritime war. Even if not taken into the employ of the Government, they would command most remunerative freights, for, until hostile Cruisers were disposed of, they would be the only ships which could safely venture on a long voyage without convoy, their safety lying in the high speed and coal endurance. On whatever service engaged, these ships, on account of their great value, would probably receive some attention from the enemy throughout the war. At such a time the advantages of duplicated machinery would be much enhanced, and might haply prove scarcely inferior to the advantages of the watertight subdivision of the hull already provided and held in such high estimation.

DISCUSSION.

Mr. W. H. White: My Lord, I should be sorry that Mr. Linnington's Paper (although it has been read so late in the evening, through causes over which he has had no control) should pass without any discussion. There is really little to debate in the Paper, which is chiefly a paper of information put together by a gentleman, whose occupation under Mr. Wright, in the Comptroller's Department of the Admiralty, gives him the opportunity of studying the latest practice, and allows him at the same time to assist in advancing practice. I am sure these details given in the Paper as to the relative efficiency of single and twin screws, the use of force draught, and the arrangements of engines, shafting and propellers will be read with interest by all those who are charged with the designing and building of ships which have to attain high speeds. Of course, there are different ways of accomplishing these high speeds, and the arrangements made are not always identical with those which the Admiralty have adopted. The great advances in steel manufacture will enable us, no doubt, in the immediate future to do many things which have not been possible up to the present time. Everything that is done to

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diminish the waste work, and increase the speeds at which engines are run, will tend to the attainment of higher speed, and still higher speeds in vessels of moderate proportions; 18 knots for a vessel of 3,500 tons is very good speed indeed, and that was reached in the *Iris* and *Mercury*, now nearly seven years ago. I hope, before the year is out, that that speed will be reached or possibly exceeded in vessels not half that size.

Mr. A. E. Seaton: Mr. Linnington called Mr. White the father of twin screws. I have not attended this Institution for many years, but when I was here before, the late Mr. Dudgeon used to have that appellation, so that I suppose now he has become the grandfather of twin screws. Mr. Linnington is evidently not aware that the twin screw has been already developed in the mercantile marine, and that in comparatively large ships. I speak with a very considerable amount of diffidence upon this matter, because what we call a big speed, Mr. Linnington would very likely call a slow speed. Two years ago we constructed two twin screw ships for the Great Eastern Railway, to run between Harwich and Antwerp, and those ships, although attaining a good speed, showed extremely low co-efficients. We were very much disappointed. The cause I need not go into now, and I merely mention it to show that the experience of the mercantile marine does not quite agree with that given by Mr. White last year. We have built during the last two or three years seven twin screw boats, some for the South Eastern Railway, some for the Great Eastern Railway, and some for the Manchester, Sheffield, and Lincolnshire Railway. All those boats have one peculiarity, and it is worthy of notice—and if it had not been for that, I do not think I would have risen at this late hour—that is, the uniformity of speed maintained by them in any weather. The Great Eastern Railway boats run from Antwerp to Harwich, and are timed to arrive at Harwich with their paddle boats which sail from Rotterdam. Their newer paddle boats are quite as fast in smooth water as the twin screw boats, and it is not an uncommon thing on a fine night for the paddle boat to be either neck-and-neck or a little before the twin screw, but directly there is a little bit of hubbub at sea, or a head wind, or even a beam wind, the paddle boat is always late, while the screw is only a very little behind her time. In fact, in the gales of the winter before last, those boats, in the stormiest weather, were rarely more than one hour late at Harwich, whereas the paddle steamers were four to eight hours late. The same remark applies to the boats running from Folkestone to Boulogne. I am told that those little twin screw boats will go out on a winter's night and make a fairly good run, when the paddle boats dare not venture outside the piers. I was myself on the first voyage of one of the Manchester, Sheffield and Lincolnshire boats from Grimsby to Rotterdam. We ventured out in the Humber when many other steamers were putting back. It was very bad weather, but I can say that the little boat behaved in a most remarkable way, the engines rarely raced, and the racing was of so little consequence that the governor was not used the whole of the night. It seemed as if when one engine wanted to race, the other took the steam from it; at all events, there appeared to be very great uniformity of motion in the screws throughout the night. The Dutch pilot took us over the bar when there was really not water enough, and after running in some way on the top of a big wave it left us behind, and we came with a bump on the bar, the following wave went clean over the stern and caused the ship to broach to and go over on her bilge. In that encounter with the bar we carried away one propeller, or about two-thirds of each blade of the port propeller. On arrival at Rotterdam, we held a little consultation as to whether we should dock

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the ship, and fit a spare screw. But I told the Captain I would guarantee that she would do at least nine knots in her crippled state—the ship had been going at 12½ knots with both screws. On the return voyage the weather was finer; we overtook ships which had left before us, and we maintained during the voyage back an average speed of 11¼ knots. That, I think, bears out the views which Mr. Linnington has put forward, as to the advantage of the twin screw for our Atlantic boats. I do not quite agree with the Admiralty in the method they adopt of fitting the stern shafts with loose couplings. Mr. Wright is, perhaps, perfectly aware that such shafts have proved dangerous. A shaft so fitted in a ship lying in Hong Kong Harbour drew clean out of the coupling, and was only discovered on its being seen that the ship was sinking. That was with a single screw boat, the screw being outside the rudder. The same thing is quite possible with a double screw. In the case of the Great Eastern Railway Company's ship we adopted a better and at the same time a much safer plan. The great difficulty in long shafts of this kind is to withdraw them, when required for examination, and for renewal of the bush. We simply increased the diameter of the inner end of the outer shaft, so as to form a socket into which the next shaft fitted, and was secured by a cross key and feather key the same as the screw boss is secured. That seems to my mind a much safer plan than the one shown. Of course it is a detail, and there may be a difference of opinion about it. I think myself that there are many other advantages which would accrue from the fitting of twin screws to large Atlantic boats. Not only would they be safer, inasmuch as they have duplicate propelling and duplicate steering power, but there is something quite as important, they have also mechanism consisting of smaller and more easily managed parts than are at present contained in large swift ships on the Atlantic.

Sir Edward Reed: My Lord, I would just like to say one word on this question. Mr. Seaton has pointed out a discrepancy between the experience of the Admiralty with twin screws, and his experience of the mercantile marine. I think the cause of that is to be found in this circumstance, that the Admiralty, in the *Devastation*, and in other cases which have become notable in connection with performances of twin screws, applied those screws to deep draught ships, whereas in the mercantile marine, hitherto they had been applied generally to light draught vessels. Therein lies the whole difference in the performance. Mr. White, in his Paper read long ago, referred to the fact that when you have a deep draught ship, and applied twin screws, and kept them well down, you then have the most beneficial results, and some advantage as compared with a single screw. I happen to know that the minds of some of our great ship owners have been much turned to the question of twin screws of late, but their great difficulty is with the docking of their ships. They are afraid of them passing in and out of the docks. That suggests to me the desire to know what is happening with regard to a system which, I think, Messrs. Harland and Wolff are carrying out, in which the twin screws are brought nearer together, their paths crossing each other.

Mr. White: It was Messrs. Thompson's.

Sir Edward Reed: Thompson's, on the Clyde, but I think Harland and Wolff also, of course, the dead wood is cut away, to allow the screw to pass the middle line, from either side. I do not know whether Mr. Linnington could tell us the result of that, but if that system should prove effective, and

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without serious defects, it will, I believe, be the cause of the twin screw becoming very generally adopted, as Mr. Seaton says, in large Transatlantic steamers, and for other purposes.

Mr. E. A. Linnington: My Lord, I would like to thank the Meeting for the attention given to this Paper, and to thank those gentlemen who have taken part in the discussion at this late hour. I am much obliged to Mr. White for the notice he has taken of this Paper. With reference to Mr. Seaton's remarks, I believe there has been no difficulty with these shafts or loose couplings in the Admiralty service. [Mr. Linnington proceeded to explain with the help of his diagrams]. The first inward coupling is a loose coupling, which is keyed to the shaft, and further secured by means of screws tapped into the shaft and coupling, or by means of a covering ring, so that when the screw works astern the shaft cannot be drawn out of the coupling. With respect to the poor results obtained in some twin screw ships, Mr. Seaton did not mention the draught of water.

Sir Edward Reed: Fourteen feet.

Mr. Linnington: Any result obtained on such a draught cannot be held to tell against the use of twin screws in the high speed deep draught merchant ships to which this Paper refers. Sir Edward Reed has dealt with this point. I am unable to give particulars of the performance of the twin screw ships with the screw discs overlapping, but probably the result would not be so good as that attainable with the twin screws in the usual positions, shown on the diagram (Fig. 3). These, I think, are all the points raised in the discussion.

The President: Gentlemen, I beg on your behalf to thank Mr. Linnington for his Paper.

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ON THE JOY GEAR IN ITS BEARING ON THE QUESTION OF SAVING SPACE IN THE ENGINE ROOMS OF WAR AND OTHER SHIPS AND ITS SPECIAL ADAPTABILITY TO TRIPLE EXPANSION ENGINES.

By David Joy, Esq., Member.

[Read at the Twenty-sixth Session of the Institution of Naval Architects, March 27th, the Right Hon. the Earl of Ravensworth, President, in the Chair.]

In bringing this subject before the Institution of Naval Architects this evening, I am aware that I shall be treating very much of what is only one of the details connected with the mechanical section of Naval Architecture. Yet my apology must be, that as all general arrangements are made up of correlated details, the final whole must depend in its completeness on the perfectness and adaptation of its details, that I trust I shall not waste your valuable time in bringing before you the individual detail which I have made my study now for some years, specially treating it in its bearing on the saving of space in ships, whether for war or for the mercantile marine, space which is thus left at the disposal of the naval architect for the hundred and one purposes for which he will find it available.

Although this valve gear has never yet been brought before this Institution at any of its Meetings, it is not my intention in this paper to go into any lengthened description of it, or to take up the valuable time of the Meeting by going over again what has already been brought before other societies, and what it is presumed is probably familiar to such members as are interested in the subject of valve motions.

It is intended rather, after glancing at a few of the results obtained by its application, so proving in the first place its special fitness for the use intended, then to proceed further to describe some of the modifications, developments and extensions in the same direction which have arisen out of its application where the saving of space has been a paramount object to be gained, and where by its employment in the triple cylinder and triple expansion engine it has enabled that engine to be placed in the same room usually occupied by the ordinary two-cylinder engine.

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First, however, as a basis to start from it will perhaps be well to give a very short description of the gear itself as originally arranged for the ordinary overhead marine engines.

The principle on which it rests is the fact that the movement of a valve for giving expansion by the use of lap, if analysed, will be found to consist of two distinct parts, namely, that part of the travel of the valve which is equal to the lap and lead together, which amount of movement must be constant under all conditions; and that part of the motion which going beyond and being extra to the former, provides for the port opening, and this requires to be adjustable to provide for control over the points of cut-off or expansion; and for reversal.

These two movements are across each other, as to time, the one being synchronous with the reciprocations of the piston, and the other across, or at right angles to this, that is, synchronous with the vibrations of the connecting rod.

Carrying out this principle, then, the mechanism to accomplish the above ends consists of a lever A (Fig. 1, Plate XVI.) pinned to the connecting rod Z, through a parallel motion BB (to eliminate the error of the radial arc of such lever). To the other end of this lever at C the valve spindle D is attached.

This lever thus transmits in proper ratio the reciprocating motion of the piston direct to the valve, giving it the amount of movement due for lap and lead.

The centre E of this lever, however, has another and independent motion imparted to it by the vibration of the connecting rod to which it is attached, and this motion guided in a curve (either by a curved slot or by a sling link) provides for the port opening in any degree, and for forwards or backwards going, accordingly as it is angled to either side of the horizontal.

Thus by one and the same piece of machinery, a lever pinned to the connecting rod, with its centre moving transversely in an adjustable path, both parts of the movement for the valve are provided; the one being constant and the other adjustable for forward or back going, and for all grades of expansion.

This form of valve gear has been fitted to a considerable number of engines, both for paddle and screw steamers, and gives so correct a distribution of steam, and also so smart

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an opening and closing of the ports, and so slight an increase of compression when linked up to high degrees of expansion, that it may be used without an independent expansion valve and gear in war and other steamers where it is often required to run the engines at a very high degree of expansion, and where with link gear it is usual to employ an additional expansion valve and its extra complicated gear.

The Diagrams Fig. 2 (Plate XVI.), are copies enlarged by camera from the ordinary diagrams from the engines of H.M.S. *Amphion*, of 5,000 horse-power, fitted with this gear, and built by Messrs. Maudslay, Sons & Field.

In taking these diagrams, which are from the high-pressure cylinder, the "cut-off" had been reduced in both cylinders to ·44 of the stroke, and it was further reduced in the high-pressure cylinder by gradations, as shown by the diagrams, till it amounted to only ·15 of the stroke, when, as will be seen, the compression had not been seriously increased, the engines turning the centres at this cut-off as quietly as at full speed.

Passing now to the modifications which have arisen out of the requirement of adaptation of the gear to various types of engines, the writer recurred to his original idea, as stated in the Paper read at Barrow in August, 1880, before the Meeting of the Mechanical Engineers' Institution, and turned his attention to the employment of the air-pump levers of a marine engine to work, or rather to assist in working the valves, for the air-pump lever gives but one of the portions of movement required by the valve, that for lap and lead, which is constant. The other portion which may obviously be obtained from the vibration of the connecting rod presents so many difficulties, and involves such complication in its application, that after repeated attempts extending over not only months but years, the writer turned to the second cylinder, say of an ordinary two-cylinder engine with cranks at right angles, and there ready to hand found the transverse movement required; so there arises from this combination a simple valve motion taken from parts of the engine already existing.

The required movements only need to be combined. And this is shown in diagram Fig. 3 (Plate XVI.), where the black lines represent the forward engine and the dotted lines the after engine.

The fixed centres are also shown in black, while the moving centres are given in circles; the letters are also made to suit corresponding parts in the other gear as far as possible.

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In this arrangement a curved link or quadrant E is carried by its centre on the air pump lever A at a point where its movement is sufficient to provide for lap and lead for the valve, and the valve link or spindle D is connected to this and is capable of being shifted to either end for reversal; this quadrant gets a rocking motion to provide for port opening from a lever F keyed on the shaft of the air pump lever of the other engine, which is prolonged for the purpose. Thus the two movements necessary to work the valves are provided by the two sets of air pump levers combined to give the required action.

The after engine valve gear is just the counterpart of this, and according to the position of the fixed and moving centres, the combinations may be made to work either two ordinary flat valves, or a flat and a piston valve.

Advancing a step further and carrying the principle to its application to three cylinders, or rather to three crank engines, it will be evident that the same combination will not apply, as there are no two motions at right angles; yet a similar and probably simpler combination arises.

Thus, that with three crank engines with their cranks set at or about 120 degrees apart, any two cranks give the same motion as to time, in respect of the third crank, that two eccentrics set to work a link motion for that crank would have.

It is, therefore, only necessary to combine the action of each two cranks respectively to work an ordinary link for moving the valve of the other cylinder, and so all round, the same parts serving over again through a part of the system.

Fig. 4 (Plate XVI.), shows this combination.

The air pump levers, or similar levers specially employed, have their centres arranged to suit the attachment to the links, which are swung by each end to the lever giving the required motion.

In the diagram the front engine is shown in full black lines, the next one in dotted black lines, and the farthest engine in lines dotted thus _.-.-.-

Following still further the investigation of the relative movements of the three cranks of a three-crank engine, and keeping in view the fact that any two cranks will provide the required combined movement for the valve of the third cylinder, it results that this method

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of combination may be still further simplified, for it follows that not only will the two cranks provide a valve motion for the third cylinder, but any two valve motions being provided it is only necessary to combine these two and a valve motion results which is suitable to work the valve of the third cylinder. There arises thus the combination shown in Fig. 5 (Plate XVI.). This figure shows the engines fore and aft, while the others represent end views. Here, 1, 2 and 3, are the three cylinders of a three-crank engine, and the valve motions of Nos. 1 and 3 are represented by their valve spindles DD and CC. It is only necessary to connect these by the floating lever L, and at the centre M of that lever will be found the correct action for the valve of the centre cylinder.

This order, however, may be varied, the principle remaining the same, viz., that the valve motions of any two, give the valve motion for the third; and this, of course, remains so, whatever may be the form of the valve motions employed for the initial movements.

Further, this resulting motion for the middle valve will always be the same as the two originals, but shorter, or it will be a mean between the two; thus if one is set to cut off at, say, 75 per cent. of the stroke and the other at 55 per cent., the resultant will be 65 per cent.; and in cutting off and following round to the back going the same relative proportions will be maintained.

Meanwhile the cut-offs of the original gears may be varied relatively within certain limits to equalize the power developed by each cylinder, and the resultant or middle motion retains its integrity.

Referring now to what has been described, it is not claimed for any of these combinations that any one will give all the desiderata that the naval architect or the ship-owner may ask for—all in one: it requires no argument to prove that you cannot have the most perfect and refined machine combining all improvements and all alternatives and yet at the least cost. But among them these combinations include an elasticity of adaptation that may well be expected to meet all reasonable cases.

It will have been noticed that throughout these details of arrangement the general principle prevails that the valves are in all cases brought out from between the cylinders, or in the line fore and aft with them, and are placed either in front or behind them in the transverse centre line; this arrangement allows of the cylinders being brought together as nearly as the flanges of the cylinders will permit; and thus arises the arrangement shown in Fig. 6 (Plate XVI.), where in the upper

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figure an ordinary two-cylinder engine is shown, compared in the lower figure with an engine with same diameter of cylinders on the new system. In the same way in Fig. 7 (Plate XVI.), is shown plan view of triple-expansion engines arranged in the ordinary form in the upper figure, while in the lower the same cylinders are shown arranged on the new system, in each case showing a saving of room of about 25 per cent., a saving worth a good deal of scheming, and which is not gained by any crowding of parts or complication of machinery, as by the new system the working parts are more accessible, and the main bearings are not in any way encumbered as in the link gear by eccentrics. In each of the plans shown those of the ordinary engines are not fancy pictures but are taken from existing engines.

Thus either a ship may carry her engines in less room, leaving the space saved available for coal or for additional cargo, or in the same space she may carry larger and more efficient engines, which is perhaps the direction in which we should rather look, both for war ships and for ships of the mercantile marine; especially in these days, when the chief demand is for excessively increased speeds on the one hand, and for high rates of economy on the other—an object probably only to be attained by the employment of the triple-expansion engine.

Referring now to our starting point, I think it will be seen that although we have been treating only of a detail of mechanical construction, the influence of the treatment of that detail has been such as to alter the whole general arrangement of the engines, permitting them to be placed in a considerably smaller space; this influence, extending and widening, affects the ship also by the space saved.

Of course, following the alteration of a detail which results in such a change, come the alteration of other details, and the question may next naturally arise as to the treatment of the crank shaft; this question, however, has already been fully treated and practically met. But this Paper has already extended to the limit to which the writer can trespass on the indulgence of the Meeting.

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DISCUSSION.

Mr. J. Wright: My Lord, I only just wish to mention, and I think it is fair to Mr. Joy that I should do so, that valve gear on his plan was applied to the engines of the *Amphion*, and that it has worked in a very satisfactory manner. These are fast running twin-screw engines which make nearly 100 revolutions per minute, each developing 2,800 horse-power. The ship has only made a voyage from Pembroke to Devonport to be fitted out, but she has also done her contractor's trials, and on those occasions the valve gear was everything that could be desired. The copy of the diagram which Mr. Joy shews, I think displays a very satisfactory arrangement. There is certainly some advantage in shortening up the engines in cases where it is desirable for other reasons to get the valves placed on the top of the cylinder, that is to say, especially for horizontal engines. There are a good many of these link motions in the market at present, and as one of the first and most valuable of them, I am glad to have been able to make a good trial of Mr. Joy's plan.

Mr. W. Boyd: My Lord and Gentlemen, I should like to occupy the Meeting for one moment in bearing my testimony to the success of Mr. Joy's gear in some engines lately fitted up by my Company, a photograph of which is on the table before you. We have only had a contractor's trial at present, but they are triple expansion engines with a piston speed of about 530 feet a minute, which in marine practice is pretty fair, and as far as we have seen at present, they are likely to give a very good result. I think Mr. Joy has had a particularly good field in the 3-crank engine, because a reference to figure 7 shows in the most graphic way the reduction of the space occupied by the machinery in a 3-crank engine in a merchant ship, and where space is a dividend earning element that becomes a very important consideration. I think that Mr. Joy's gear, so far as I am able to judge of it at present, seems likely to fulfil conditions which will make it very valuable in the mercantile marine.

Mr. G. Duncan, of Maudslay's: My Lord, I think it is due to Mr. Joy to say, that the case of the *Amphion* was about as bad a case for his particular gear as could be well imagined. We were very much confined in length and space, hence all the levers had to be adjusted to meet our views and not those of Mr. Joy. I think it only fair to say this, for although we obtained the indicator diagram exhibited under those disadvantageous circumstances, it is presumable that we should have got a much better one, if Mr. Joy had had it all his own way. We have at the present time two large engines of 10,000 indicated horse-power, one set of which will be ready for its trial in about 9 months hence in Italy—they are fitted with Mr. Joy's valve gear, but instead of the sliding block and curved slot we have adopted in preference the arrangement with a radius rod, which produces nearly the same motion. I think this will be more satisfactory, and that we shall be able to provide Mr. Joy with another paper at some future time. I will not detain the Meeting any longer at this late hour.

Mr. D. Joy: My Lord and Gentlemen, I am sure that at this late hour of the evening I shall best consult your pleasure and your interest by not indulging in any lengthened remarks, yet in courtesy to those gentlemen who have so kindly spoken in favour of my system I must say a word of two. Taking, then, Mr. Wright's statements as to the results obtained from the engines of H.M.S.

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Amphion, coupled with Mr. Duncan's remarks as to the same engines, which I fully endorse and repeat, I wish to draw your attention to his statement that the indicator cards here shown are from engines with exceedingly short connecting rods, so placing the valve gear at a great disadvantage as to the accuracy of the action, indeed placing it in about the worst condition in which it could be applied; yet from the great care with which it was worked out in Messrs. Maudslay's drawing office, under Mr. Sell's supervision, we obtained the very satisfactory results shown by the diagrams. Sharp and clear "admission" and "cut-off," and only a very slight increase of "compression" when carrying the "cut-off" from ·44 down as far as ·15. With the larger engines of which Mr. Duncan has spoken, where the conditions are more favourable as to length of connecting rod, we shall have even a more perfect result; as in engines for the mercantile marine, where saving room is not so much a first object, and a reasonable length of connecting rod is admissible, the "cut-offs" are almost mathematically correct. Although the cases referred to by Mr. Duncan and Mr. Boyd have as yet only run their trial trips, I think anyone will admit that an Admiralty trial trip of "six hours full speed" is no child's play, and to run such a trip as the *Amphion* did without hitch of any kind is good guarantee for the future; but beyond these I may name that I have now a considerable number of engines from 500 to 1,200 I.H.P., most of which have been running from two to three years with perfect satisfaction to their owners, and those engineers and owners who have adopted or built the system are continuing to adopt and build it. I may here remark that the test of endurance is very differently estimated by the makers of different classes of engines. Thus the builders of marine engines tell me that such work is the most severe test for any machinery, while locomotive work is comparatively easy, yet the locomotive builders assure me that the excessively high speeds of locomotives must be the most severe test of endurance, and yet I have quadrant blocks out of such engines which have run 48,000,000 revolutions and have not been lined up. My impression is, that the engines of steel rolling mills, where the work is practically constant "racing" is the most severe work, and yet I have found this gear work satisfactorily day and night continually on such engines, indicating an average of 1,200 to 1,400 H.P. Another feature of this valve gear, of which I was reminded an hour ago by Mr. Pilcher, of Liverpool, who has had the engines of the *County of Salop* running now for some time, is this; that they are so sensitive, and yet so perfectly under control, that although he allows none of his other ships with link gear eng nes to employ steam in dock, the *County of Salop* is always handled under steam, the captain or pilot always being able to rely on the exact order given being carried out, the engines moving ahead or astern, or a part of a turn, instantly. This was an advantage I urged long ago, and have constantly witnessed its effect both with marine engines and locomotives, where it is very evident in the smart start they will make, never requiring to be reversed to get them to go ahead. Now only one word more, which is, I think, made necessary, as from the lateness of the hour, it was found advisable to omit the reading of some portions of the Paper, the continuity of the idea which I have been endeavouring to work out appeared to have been somewhat deranged, let me repeat that continuity very shortly. I began by designing a valve gear taking motion from the connecting rod, utilising the two motions of reciprocation and of vibration on the one engine. Next I got a similar result by utilising the reciprocating motion from the one engine and that corresponding to the vibrating from the other engine, going on then to three-crank engines I get the motion for the valve of each cylinder from the reciprocating action of the other two through their air-pump levers. Still further,

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in the same class of engines, employing a complete valve motion to two of the cylinders, I combine these by a floating lever, and so produce the valve action for the third cylinder, thus saving a whole set of valve gear; and I am continuing my investigations in the same direction, with a very tangible hope that I shall shortly be able to take a step further in the simplification of valve gears by about 30 per cent. further. And in view of the prospect of greatly increased speeds of revolutions and much increased development of power, I think the greater the simplicity that can be attained the better.

The PRESIDENT: Gentlemen, I am sure you will allow me to return your very best thanks to Mr. Joy, and perhaps I may be allowed, on your behalf, to express regret that our Friday evening time is always rather short, and having had a great deal of work to do to-night, that time has not permitted us to go fully into the merits of Mr. Joy's admirable invention. I know that he must have taken an immense deal of pains over it, and certainly we should not withhold any meed of praise that he has earned.

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A DIAGRAM OF STABILITY.

By Captain Perez Seoane, R.S.N. (Naval Architect), Member.

[This Paper was received too late to be included in the Programme of the General Meetings, but is published in the Transactions by order of the Council.]

In this Institution—last year's meetings—there was expressed by more than one of its most illustrious members, the regret that when the science of the naval architect had so much advanced, no steps seemed to have been taken in the direction of somehow, graphically or otherwise, collecting and embodying all the principles of Naval Architecture directly connected with the safety of vessels when in the waves, in such a way that the person in charge of a vessel should have the possibility of knowing of his vessel so much as is necessary for ascertaining her safety under whatever conditions, and with the correctness and accuracy possible at the actual stage of knowledge in these matters.

The process described in this Paper is but an intended step in the said direction.

The special graphical representation proposed in it for bringing into view all the geometrical data of a given vessel, having anything to do with her statical transverse stability, and consequently with her safety when among the waves, has the properties—(1) of being applicable to any practical draught of the said vessel and to inclinations up to 90°, and (2), what I believe most important, of presenting them in the position they obtain in the real vessel, thus making them as telling to the senses, we may say, as to the mind, and thus of real practical value to persons not thoroughly acquainted with mind-distorting diagrams. The special graphical representation or diagram proposed is simply this: the midship section of the given vessel, on which section four curves of centres of buoyancy are drawn, two corresponding to the maximum and minimum practical draughts, and the other two to intermediate draughts, the difference of displacement from each one to the next being the same; they extend from the upright to 90° on one side; by such a diagram, and as long as we know the position of the centre of gravity, righting levers, and therefore righting couples may be readily obtained for any inclination within a range of 90° from the upright position, as well as dynamical stabilities between any two inclinations within the same range, for any of the four displacements corres-

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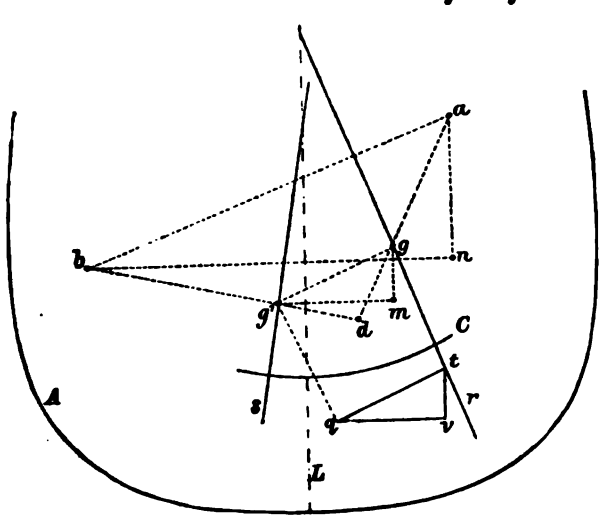
ponding to the four curves of centres of buoyancy drawn on the midship section; because we know that the righting lever for any inclination will be given by the distance of the corresponding centre of buoyancy to a straight line drawn by the centre of gravity and forming with the middle line the angle of inclination given, and that the geometrical factor of the dynamical stability between any two inclinations is given by the difference between the distances from the centre of gravity to the two righting levers corresponding to the two inclinations and drawn by their corresponding centres of buoyancy. An instrument may be easily imagined to promptly make these measurements; it is essentially but a square angle one of whose arms is made to pass by the centre of gravity and form with the middle line the angle of inclination we are dealing with, and the other arm made tangent to the corresponding curve of centre of buoyancy, or simply to pass by the centre of buoyancy for the corresponding angle of inclination as the said curves have their points marked with the inclination whereat they actually come to be centres of buoyancy. For displacements intermediate between any two of the four directly considered on the proposed diagram, we will have first to locate the corresponding centre of buoyancy, which we can easily do by availing ourselves of the known position of the centres of buoyancy for the same inclination, and for the two contiguous displacements out of the four considered in the diagram, and between which the present displacement stands, and of the constancy of ratio within the limits of practical draught between increments of displacement and raisings of the centre of buoyancy for a constant inclination, and taking—which we may for practical purposes—as a straight line the stretch of a curve of loci of centres of buoyancy for a given inclination and different displacements, between two contiguous curves of the four drawn in our diagram; once the centre of buoyancy is thus located, we may deal with the case as before.



As for the locating of the centre of gravity, let us suppose a vessel whose midship section is A, L the middle line, P the displacement, and C the curve of centres of buoyancy for the said displacement P; let us suppose that the stowage has been altered by the moving of a known weight p from a to b, and that d is the centre of gravity of the vessel where the weight p is taken off altogether. The three points, a, b, d, and the two resultant centres of gravity, g g^1, (g when p is in a, and g^1 when it is in b), will form

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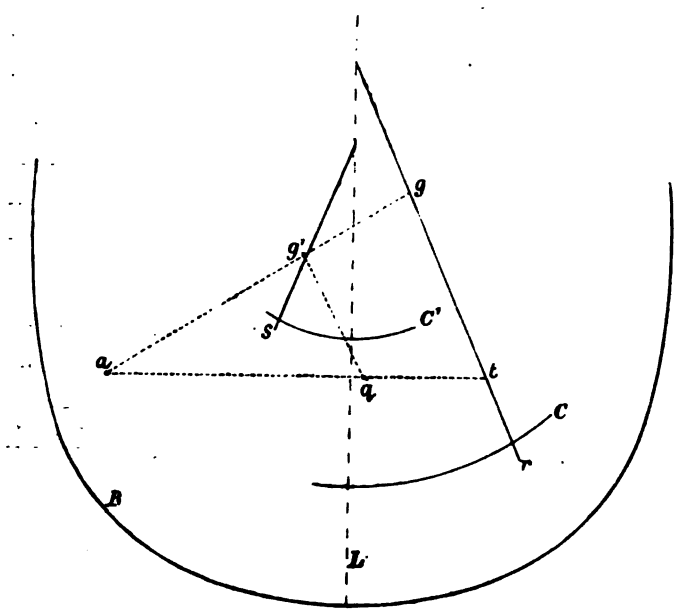
the figure $a\,g\,d\,g^1\,b\,a$, in which g is on the straight line $a\,d$, and g^1 on the straight line $b\,d$; $g\,g^1$ being parallel to $a\,b$; if we draw $a\,n$ and $g\,m$ parallel to L and $b\,n$, and $g^1\,m$ perpendicular to them, we have it that $\frac{g\,m}{a\,n} = \frac{g\,g^1}{a\,b} = \frac{d\,g}{d\,a} = \frac{p}{P} \therefore g\,m = \frac{p}{P}\,a\,n$

$$\text{and } \frac{g^1 m}{b\,n} = \frac{g\,g^1}{a\,b} = \frac{p}{P} \qquad \therefore g^1 m = \frac{p}{P}\,b\,n$$

If we have the inclinations taken by the vessel when p was at a, and when p was at b, and have means to draw the normals r, s to C at the centres of buoyancy corresponding to the two inclinations, we can, after measuring $a\,n$ and $b\,n$, and multiplying them by $\frac{p}{P}$, construct the right angle triangle $g\,g^1\,m$; if this triangle be placed in a position like $q\,v\,t$, so that t be on the straight line r, and $t\,v$ parallel to L, then, if the triangle be moved upwards parallel to itself, keeping always t on the line r, until q comes to be on the line s, then this last point (position taken by q on s) will be the centre of gravity looked for.



Let us suppose now a vessel whose midship section is B, L its middle line, C the curve of centres of buoyancy for the displacement P with a given cargo, and C^1 her curve of centres of buoyancy when her cargo is added to by a known weight p; supposing g to be the position of the centre of gravity before p has been added, a the position taken by the centre of gravity of the additional weight p, and g^1 the resultant centre of gravity after p is added at a, we have it that g^1 will be on the straight line $a\,g$, and that drawing $a\,t$ perpendicular to L, and $g^1\,q$ parallel to r, which is the normal to C, at the centre of buoyancy before p is added, we may write

$$\frac{q\,t}{a\,t} = \frac{g\,g^1}{a\,g} = \frac{p}{P+p} \therefore q\,t = \frac{p}{P+p}\,a\,t;$$

thus we may mark on $a\,t$ the point q, and drawing by it $q\,g^1$ parallel to r, determine by its intersection g^1 with s, which is the normal to C^1 at the centre buoyancy after p has been added at a, the resultant centre of gravity g^1 looked for.

Thus, if the diagram proposed affords means for drawing the two normals r, s, we see that by this process the position of the centre of gravity may be known for any alteration

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or addition in the stowage without interfering with the expedition with which the operation of loading must be done in merchant vessels, it only being necessary that the person in charge of the loading shall have the load so distributed that at some stage near the end of the loading an appreciable inclination shall obtain and be measured, and that the space still empty at this stage and remaining to be filled up with the rest of the cargo be such that the distance of its centre of volume to the keelson top and to the diametral plan of the vessel can be approximately guessed. (It is assumed that the portion of the cargo left for this last stage of the loading is homogeneous in its nature.)

Generally the point g^1 will not be on L, but we may assume that the slight alteration required for bringing it on to L will consist of moving a small portion of cargo perpendicularly to the diametral plan, thus the height of the g^1 already found will not change.

The diagram proposed in this paper gives at once the centres of buoyancy for any inclinations and any of the four displacements assumed on the four curves, and we said before how easily any centre of buoyancy referring to any intermediate displacement can be found. The drawing of the normals is reduced to the drawing of a straight line, making with L the corresponding angle of inclination; thus we see that the operations required for locating the centre of gravity are anything but difficult or complicated.

It may be deemed convenient to know for any inclination and displacement the position taken by the line of flotation; the diagram proposed may be made serviceable for this purpose by adding to it the four curves tangent to all the flotations, from the upright position up to 90° of inclination, each of them corresponding to each of the four displacements assumed in the four curves of centres of buoyancy, and represented in the midship section in the relative position they obtain in the vessel. The position of any flotation for any displacement other than the four of the curves may be easily and correctly enough found by an instrument which may be essentially described as a jointed parallelogram; two opposite sides of the parallelogram are to be placed forming with L the angle of inclination (with L) of the flotation we want to know, and tangent to the two adjacent curves between which the curve similar to them but corresponding to the intermediate and actual displacement would be, if drawn; then dividing the other two sides of the jointed parallelogram proportionately to the difference between the actual displacement and the two out of the four assumed in the four curves, and adjacent to it, the straight line passing by the two division points, which will be parallel to the other two sides of the parallelogram, will represent the line of flotation.

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CONCLUDING REMARKS.

Mr. B. Martell: Our Meetings having come to a conclusion, I have great pleasure in proposing what I am confident you will cordially endorse, and I feel at the same time that it is a privilege I enjoy in being permitted to do so, that the sincere thanks of this Institution be returned to our noble President for the very satisfactory manner in which he has presided over us. Fortunately, we have in his lordship one who has been associated with a great maritime port, which has enabled him to cultivate a natural taste for shipping matters, and I may say that the shipping community of this country on many occasions has experienced very great benefit from the knowledge which his Lordship has on the subject. At the same time, combined with that knowledge, he has all the qualities which render him so essentially able to preside over the meetings of an Institution of this kind. He possesses that courteousness of manner, that kindly consideration and regard for the feelings of everybody, that desire to encourage the efforts of the young rising members of this Institution, while at the same time possessing that firmness of character which enables him to keep all the members under proper control, and to preside over the deliberations of this Institution in the very best possible manner.

Mr. W. Denny: I have very much pleasure in seconding the vote of thanks which has been proposed by Mr. Martell, but I would have had more pleasure in doing so if he had not exhausted the ground in which I have to work. It is very difficult to come after Mr. Martell, and to add to what he says, therefore I think the best thing I can do, instead of attempting to add to, is rather to slightly develop what he has said. In a President for such an Institution as this we have to look for attainments and deficiencies. It seems a strange thing to say that we have to look for deficiencies, but it is true. We need a President who has a certain intimacy with the studies which we are pursuing, but not what may be called a close technical intimacy with them. A President having a very close technical intimacy with the subjects of discussion would on some occasions be tempted to take a side. In Lord Ravensworth we have a Pesident who has precisely that amount of knowledge which we require, and all the interest we ourselves feel in our professional subjects, without his being a technical expert. He has served upon many Commissions and Committees of importance; and we have reason to congratulate ourselves that the Government, within the past twelve months, have given a fresh recognition of his fitness for such honourable work in appointing him the Chairman of the most important Commission on naval expenditure ever convened in this country. Over and above the interest and information of which I have spoken, Lord Ravensworth possesses other and essential qualities to which Mr. Martell has referred—he has all the fairness, the firmness, and the courtesy required for the control of our Meetings; Mr. Martell has, besides, laid his finger upon one most important point, in directing our attention to the interest which his Lordship takes in every attempt made by the younger members of this Institution to come to the front. I remember very well the first occasion on which I spoke here. I occupied a back seat, very near the wall, and I possessed at that time even a stronger Scotch accent than I now have. I was ordered to sit down three times in succession before I was allowed to speak, and there were some unkind people near me who said, "Serve the Scotchie right." I do not think in this Institution there is now the slightest probability of any young fellow rising to speak for the first time and receiving such treatment. I am certain Lord Ravensworth would welcome a strange voice, accompanied by a youthful face and youthful audacity. I, therefore, add my tribute of esteem and the expression of my thanks to those of

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Mr. Martell, and I ask you to unite in his most hearty vote of thanks to Lord Ravensworth for the way in which he has conducted these meetings.

The PRESIDENT: Mr. Martell, Mr. Denny and Gentlemen, I cannot well express to you—I have not the words or the powers—my grateful feelings at the reception you have given to the proposition of those two old friends of mine. I thank you with all my heart. All I can claim to do here is to be as fair as I possibly can by every man, and at the same time to observe strictly, but not with extreme severity, the rules so prudently laid down for the guidance of the proceedings of this great Institution. Those are my main duties, and I am glad to think that I may possibly have satisfied you in those two respects. Now, Gentlemen, I must be allowed, although at this late time, just to give expression to one or two thoughts that the proceedings of this year have suggested to my mind. I congratulate you most cordially upon a most successful meeting to begin with, I have often felt the value of an Institution of this kind, but I never felt it more strongly than I do upon the present occasion, because this is an Institution which essentially adapts itself to the requirements of the times, to elucidate questions upon which the public mind may not be thoroughly informed, and certainly not thoroughly made up. Now, let me illustrate to you by reference to three instances what you have been doing in the course of this week. You have had a most able paper upon a subject that is of the first importance to our steamship owners in this country. I mean the subject of stowage, and it is the first time that a paper has been expressly devoted to that purpose since the Institution was founded. It is especially important in connection with that which we are all waiting to hear with great anxiety and with extreme interest, namely, the Report of the Commission over which Sir Edward Reed has so ably presided on the load line of ships. There you have met a requirement of the time by elucidating the subject of stowage in a most able paper and the discussion which followed on it yesterday. But you have gone beyond that, and had a most able paper on rivetting, and we have extracted from Professor Kennedy, perhaps the best authority on that subject, a sort of half promise, which I intend to make him comply with if I can, to bring into a *resumé*—into a concise form—the results of the long experiments which he has been conducting on the subject. You have to-night done, I believe, a great public duty; you have gone into the question of steel boiler plates, and I believe that discussion has been really a cautionary discussion, not intended to create, and I think it will not be attended with the result of creating, alarm or panic, but merely to express the caution that should be exercised in the direction of increasing steam pressure too rapidly. Thus you have performed three great public duties. I believe that the public, when they come to read your proceedings, will feel pride and confidence in possessing in its midst an Institution of this kind which is so thoroughly competent to enter into these technical details to give an expression of opinion derived from the purest sources—from men who really know thoroughly and entirely what they are talking about, and who are able to instruct the public, and, therefore, gentlemen, you are really fulfilling the high functions which this Institution was intended to perform. There is only one word which I should like to say before I part with you, which is, that I rather regret we have not had one other paper upon naval ship construction. I had hoped for one, especially with reference to torpedo defence, because that is, to my mind, the most important question in regard to naval construction that can be produced and broached at this time. I believe that in our position as a great naval power the danger of the torpedo is the greatest danger that we shall have to encounter at sea, and with great submission and with great deference I would express to this Institution, the hope that if we are all spared to

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live another year, that a paper, and an exhaustive paper, on that subject may be presented to us by one of our Members. We have several men perfectly capable to do it, and I express my very earnest wish that that may occupy the attention of some of our eminent men.

One single word more. I certainly feel it my duty to call the attention of our Council to this, that in future we should not crowd too many subjects into the Friday evening programme. We have hardly done as much justice as I should have liked to two of the Papers this evening, in consequence of the clock, which warned us it was getting rather too late, but I do not think anybody in this room will grudge a single moment spent in that important discussion of the steel boiler plates that preceded the discussion of those two last Papers, but I think we shall be easily able to arrange on another occasion not to have so much to do on the Friday evening so as not to hurry the last Papers, as it were, and not do justice to them. I have ventured to detain you with these last few remarks as they are matters of great moment, and, in conclusion, I thank you with all my heart for the kind expressions which you have made use of towards me. All I can say is, I hope we shall all meet here together another year. (Applause.)

Mr. A. E. Seaton: There is one more duty we have to perform, and it has been placed in my hands this evening to initiate. It is to me a grateful one, as perhaps my presence here this evening is due as much to the Council of this Institution as to anything else. It is now fourteen years since I attended a meeting of this Institution of Naval Architects, and the last time I was here I was here on sufferance as a student at the Royal School of Naval Architecture at South Kensington; I, at that time viewed the Council of this Institution with a considerable amount of admiration, and I looked upon them almost with awe, because among them were the heads of the two professions in this country. I am very pleased to see to-day that the Council still consists, as it did then, of the men most eminent in Naval Architecture and Marine Engineering. I miss many well-known faces that then belonged to the Council, and I regret that it has not been allowed me to see them here once more. I am sure in the late Mr. Scott Russell for one, this Institution lost a very valuable Member. I have a little book at home which I value very much, it contains the sketches I made on the occasion I allude to, of various prominent Members of this Institution. I remember the late Lord Hampton detected me in the act of sketching him so his portrait was not complete. I give that as an instance of the respect and esteem I had for the Council of the Institution then. That respect has been enhanced, I am sure, in the last few years by the very able way in which the proceedings of this Institution have been conducted. I am sure the best thanks of the Members of the Institution are due to the Council for managing the affairs of this Institution as they do, and for carrying it on for the good of the profession generally and the country at large. I am sure there is no one here to-night who will grudge giving a very hearty vote of thanks to them for the very good work they have done, and the able manner in which they have carried out their duties during the last twelve months. Therefore, my Lord, I have to propose that a vote of thanks be given by the Members of this Institution to the Members of the Council for their very able services during the past year.

Mr. M. H. Robinson: I rise with the greatest pleasure, but with some diffidence, to second the resolution put by Mr. Seaton. He has spoken so well on the matter that it remains for me to say very little. As a man of business, and one not unused to hard work, I can well understand the value of the

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time which is given to our affairs by our Council; and as one whose professional connection with the subjects dealt with by this Institution is very recent, I would like to bear witness to the very great educational value of the Council's labours. I can testify in my own person to how great an extent I owe the small professional knowledge I possess to this Institution, its meetings, and its papers. For these and for much else I for one have to thank the Council of this Institution. I beg leave to second the motion.

Mr. J. Wright: On behalf of the Council, I beg to thank the meeting for the vote of thanks which they have been good enough to convey to the Council for the labours which they have undertaken from time to time. The Council is a large one, and the Members in all parts of the country are well represented on it. I have had considerable experience on the Council now, and I may say that on all occasions its proceedings have been most harmonious, and when a number of men meet together, whether they are a Council or a Cabinet, if they work together harmoniously the results must be good. I am sure the Council of this Institution have the interests of it truly at heart, and do all they can to promote its prosperity.

The President: I must crave your attention for a single moment. We cannot part without expressing our thanks—and I trust they will be unanimous and hearty—to the Secretary of this Institution. I perhaps know more than any other Member of this Institution the value of the labours and the services of the excellent Secretary which we are so fortunate to possess; and I am sure you would have thought it a great omission if I had allowed this occasion to pass by without asking you to express your approval of the services which he renders so efficiently and ably, and so willingly to the Institution at large.

Sir Edward Reed: There is another duty which we have never failed to perform, and I hope never shall, at our final meeting, and that is to return our best thanks to the Society of Arts for the magnificent accommodation which they so liberally provide us. I really do not know—I never did know—what our claim upon the Society of Arts is; but what I do know is that, from the very first time of our meetings, when we had no position before the world at all, but had the world all before us and no home, the Society of Arts most readily and most generously allowed us the use of this magnificent room, and other rooms for the purposes of the Council in the most liberal manner. I do not know whether the time will come when we shall be obliged to cease to trespass upon their kindness. We heard at the dinner the other night that when we became so large a body that this room would not contain us, we shall have another one to which we should be welcome, because Sir Frederick Bramwell told us that the Institution of Civil Engineers would gladly open its Hall to us if such a necessity should arise. That is a very gratifying fact; but, at the same time, for old acquaintance sake, I hope we shall remain here. My doubts were raised by the immense meeting of last evening; but while we accept the kindness of the Society of Arts, I hope we shall never be wanting in gratitude for the services they have rendered us, and I propose that our best thanks be given to the Council of the Society of Arts for their very great kindness to us.

Mr. G B. Rennie: I have very much pleasure in seconding the resolution that the thanks of the meeting should be conveyed to the Society of Arts for the kind way in which they have allowed us to use this Hall.

The President: Then that terminates the Meeting.

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MEMOIR OF THE LATE MR. J. D'A. SAMUDA.

THE Council deeply regrets having to announce the death of one of the oldest Officers of the Institution, the late Mr. J. D'A. Samuda, who has for twenty-six years, in fact ever since the foundation of the Institution, discharged the important and onerous duties of Treasurer. Mr. Samuda was one of the original founders of the Institution, and was naturally elected as one of the Members of its first Council. In the year 1868 he was chosen Vice-President, which position he occupied till the day of his death. He took the deepest interest in all that pertained to the affairs of the Institution. He was seldom absent either from the Annual Gatherings or the Meetings of the Council. He was a frequent contributor to the *Transactions*, and lost no opportunity of promoting to the best of his ability and influence the interests of the Society. His death cannot but be felt as a heavy loss to the Institution.

Mr. Samuda was born in the year 1813. He was the son of the late Mr. Abraham Samuda, merchant, by Joy, daughter of the late Mr. H. D'Aguilar, of Enfield Chase. Early in life he became the pupil of his brother, the late Mr. Jacob Samuda, and the two brothers entered into partnership in 1832 as marine engine builders, but they afterwards took up shipbuilding in iron and steel. Mr. Jacob Samuda was unfortunately killed in 1844 by the blowing out of an expansion joint when trying the engines of the *Gipsy Queen*, one of the first vessels built by the firm, and the business was in consequence afterwards carried on under the management of Mr. J. D'A. Samuda. In former times, when merchant shipbuilding flourished on the Thames, the firm of Samuda Bros. built a large number of mail steamers and other mercantile vessels; but afterwards, when circumstances caused the transference of the bulk of the shipbuilding trade to the Clyde and North-East Coast, the firm was principally occupied in the construction of ships of war. Of the latter type of ship Mr. Samuda constructed a number for our own and for foreign Governments, including the *Thunderbolt*, which was one of the first iron armour-cased vessels built in this country; the *Prince Albert*, which was one of the earliest armour-clad turret ships; and, latterly, the *Belleisle* and *Orion*, iron corvettes, of 4,700 tons displacement, and 4,000 horse-power, built for the Ottoman Government, and purchased by the Admiralty. The *Independencia*, iron-clad, of 9,310 tons displacement, and 8,000 horse-power, was finished by Mr. Samuda for the Brazilians, having been begun

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by Messrs. Dudgeon, and this vessel also was purchased by Lord Beaconsfield's Government out of the Vote of Credit, and is now known as H.M.S. *Neptune*. The most remarkable vessels built for foreign Governments were the *Kaiser* and *Deutschland*, frigates, of 7,500 tons displacement, and 8,000 horse-power, built for the German Government, and which realised speeds of about 14·5 knots on their trial trips; the *Almirante Brown*, steel-faced armour ship, of 4,200 tons displacement, and 4,600 horse-power, built for the Argentine Republic; and the *Riachuelo* and the *Aquidaban*, built for Brazil. The former of these vessels is 5,700 tons displacement, having engines of 6,000 horse-power, and realised on her trial a mean speed of 16·7 knots under forced draught. The *Aquidaban* is a very similar ship to the *Riachuelo*, but of rather smaller dimensions. She was launched in January last, and is now undergoing her trials.

Mr. Samuda, at a comparatively early date, earned a great reputation as a builder of fast steam yachts and mail steamers. Of these may be mentioned the *Mahroussé*, built for the Khedive of Egypt, and the *Leinster*, built for the City of Dublin Steam Packet Co. over twenty years ago. Both of these vessels attained the very high speed, for that time, of 18 knots. Within the last few years he built the *Albert Victor* and the *Louise Dagmar* for the Folkestone and Boulogne service, vessels which are each of 1,040 tons displacement and 2,800 horse-power, and which achieved the speed of 18½ knots.

Mr. Samuda will be remembered not only as a successful shipbuilder, but also as a prominent Member of Parliament. In 1865 he obtained his first seat in the House of Commons as Member for Tavistock, and speedily made his mark, not only as an independent politician of Liberal feelings, but as an authority on all matters relating to Naval expenditure, shipbuilding, and National defence. When the Tower Hamlets was created an independent constituency, Mr. Samuda and the Right Hon. A. S. Ayrton were elected its first representatives, in the year 1868. He continued to represent this constituency till the General Election of the year 1880, when he lost his seat, chiefly in consequence of his disagreement with the heads of his party on the subject of the foreign policy of Lord Beaconsfield's Government. His defeat was greatly regretted by a large section of his old constituents, who to the number of 10,400 presented him with an address and a valuable testimonial in the July following the General Election.

On the news of Mr. Samuda's death being received a Special Meeting of the Council was held to consider what marks of respect could be most fittingly paid to the memory of one who had been such a good friend and supporter of the Institution.

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As the outcome of this Meeting a Deputation, consisting of Lord Ravensworth, Sir Edward Reed, and the Secretary, attended at the funeral, and a Vote of Condolence, in the following terms, was forwarded, on behalf of the Institution, to Mr. Samuda's family:—

"The Council have heard with much regret of the decease of their highly esteemed friend and "colleague, Mr. J. D'A. Samuda, and request the President to convey to his family their condolences, "and their assurance that Mr. Samuda's valuable and distinguished services to Naval Architecture and "to this Institution will be long remembered, and will be permanently recorded in the pages of the "*Transactions*."

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NOMINAL INDEX.

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NOMINAL INDEX.

BEING AN ALPABETICAL LIST OF THE NAMES OF THE AUTHORS OF PAPERS READ BEFORE THE INSTITUTION, AND OF THE SPEAKERS WHO TOOK PART IN THE DISCUSSIONS WHICH FOLLOWED THE READING OF THE PAPERS.

Name of Writer or Speaker.	*Title of Paper*, or Subject under Discussion.	Page.
Anonymous	Memoir of the late J. D'A. Samuda	307
Boyd, W.	Treatment of steel boiler plates at the Wallsend Slipway, Newcastle	259, 273
	On Joy's valve gear	295
Colville, —	Introducing Mr. Cuthill	259
Cotterill, Professor J. H. ...	Mr. MacFarlane Gray's ether pressure theory	163, 164
Cuthill, W.	History of the manufacture of the steel plate, referred to in Mr. Parker's Paper	260
Denny, W.	Model experiments	44
	Water chambers for reducing the rolling of merchant ships ...	44
	Mr. Heck's apparatus for mechanically measuring a ship's stability	66
	Labours of Englishmen and Frenchmen in increasing our knowledge of the science of stability	66
	Time required to calculate a ship's stability by Mr. Heck's method	68
	Stowage of merchant steamers	109
	Economy of triple compound engines	149
	Two crank and three crank triple compound engine compared ...	150
	Quadruple compound engines	150
	Mr. Parker's Paper on the use of thick steel boiler plates ...	262
	Use of thick steel for boiler making	263
	Seconding vote of thanks to the President	302
Dick, F. W.	Second law of thermodynamics	162
	Failure of the steel plate referred to in Mr. Parker's Paper ...	266
	Dangerous temperature for working steel plates	266
	Percentage of carbon in thick steel plates	267
	The working of thick steel plates	267
Dixon, Raylton	Failure of the steel plate referred to in Mr. Parker's Paper ...	268
Duncan, G.	Repair of broken stern posts	238
Duncan, G. (of Maudslay's) ...	On Joy's valve gear	295
Fitzgerald, Captain C., R.N.	Use of water chambers for checking the rolling of ships ...	46
Froude, R. E.	Explanation of model illustrating Mr. Watts's Paper	41
	Effect of variation of shape of water chambers for checking the rolling of ships	47
	Importance of studying the proper proportion of water chambers for checking the rolling of ships	47
	Effect of water chambers compared with bilge-keels in checking the rolling of ships	48
	Advantage of water chamber in securing steady gun platforms ...	48
	Captain Tuxen's proposed time allowances for yacht races ...	125, 126
	Objects to be considered in framing rules for yacht racing ...	125
	Sea going powers of modern racing yachts	126

Digitized by Google

Name of Writer or Speaker.	*Title of Paper*, or Subject under Discussion.	Page.
Froude, R. E.	Rules of the Yacht Racing Association	126
Gray, J. Mac Farlane ...	Use of water chambers for checking the rolling of ships ...	46
	The theoretical Duty of Heat in the Steam Engine	154
	Reply to discussion on his Paper	164
	Mr. Milton's Paper on marine boilers	247
	Proper method of fixing the area of heating surface in boilers	251
	Point of order	260
	Failure of the steel plate referred to in Mr. Parker's Paper ...	265, 276
Hall, J. F.	Use of cast steel in the manufacture of stern frames and rudders	235
	Stern frame of the *S.S. Martello*	236
	Manufactures of rudders	236
	Chemical analysis of steel plates	274
	Failure of the steel plate referred to in Mr. Parker's Paper ...	274
Hay, Sir John C. Dalrymple	Defence of ships against rams and torpedoes	27
	Effect of speed of punching on the strength of riveted joints ...	216
Heck, John H. ...	*Mechanical Method of Measuring a Vessel's Stability* ...	50
	Reply to discussion on his Paper	70, 72
	Behaviour of steel of high tenacity	273
	Behaviour of thick steel plates	273
Inglis, J., jun.	Propulsive effect of sails	125
	Captain Tuxen's proposed time allowance for yacht races ...	125
Joy, D.	*On the Joy gear in its Bearings on the Question of saving Space in the Engine Rooms of War and other Ships, and its special Adaptability to triple Expansion Engines*	289
	Reply to discussion on his Paper	295
Kennedy, Professor Alex. B. W.	Strength of riveted joints	211
	Hand and machine riveting compared	211
	Mr. Milton's Paper on the strength of riveted joints ...	212
Kirk, A. C.	History of the introduction of triple compound engines ...	146
	Injurious effect produced by cylinder oil in boilers... ...	146
	The economy of triple compound engines	147
	Advantages of three crank engines	147
	Extensive use of triple compound engine	147
	Boilers of triple compound engine	147
	Strength of riveted joints	220, 226
	Alternating strains in riveted joints	220
	Modern practice in the proportioning of riveted joints	220
	Machine *versus* hand riveting	221
	Rate of combustion on small grates	247
	Failures in steel plates...	263
	Chemical composition of steel shell plates	264
	Percentage of carbon in thick steel plates	264
	Failure of the steel plate referred to in Mr. Parker's Paper ...	264
	Experiment on the stretching power of tool steel	265
	Rules of Lloyd's and Board of Trade for shell plates of marine boilers	272
Laird, H. H.	Importance of using steel rivets in the joints of steel plates...	223

Digitized by Google

Name of Writer or Speaker.	*Title of Paper*, or Subject under Discussion.	Page.
Laird, H. H.	Annealing of punched holes in steel plates	223
Liggins, H.	Use of bilge keels in checking rolling of ships	41
	Use of water chambers in checking rolling of ships ...	41
	Racing rules of the Yacht Racing Association	124
	Lead ballast for yachts	124
Linnington, E. A.	*The propelling Machinery of high speed Ships*	278
	Reply to discussion on his Paper	288
Mc Lean, —	The forging of stern frames...	232
	Different sorts of welds compared	232
Manuel, G. W.	Proper form of test pieces for experiments on riveted joints ...	213, 214
	Experiments on riveted joints of steel boilers	214
Martell, B.	Inapplicability of water chambers to checking the rolling of merchant ships	42
	Tendency of merchant shipowners to unduly increase the beam of their ships	43
	Assistance given to Mr. Heck by Messrs. Denny Bros.	69
	Specific gravity of different descriptions of cargo	105
	Specific gravity of coal cargo	105
	Calculation of the carrying capacity of ships	106
	Sinking of the *Austral*	106
	Value of Mr. Nicholson's Paper on the arrangement of coal bunkers	107
	Effect of consumption of bunker coal on the safety of steamers ...	107
	Method of securing lead keels in yachts	128
	Mr. Milton's Paper on the strength of riveted joints	215
	Accidents to forged stern frames	231
	Importance of testing steel in laboratories	269
	Failures of steel plates	270
	Proposing vote of thanks to the President	302
Milton, J. T.	*Some Notes on the Strength of riveted Joints*	204
	Reply to discussion on his Paper	225
	On the efficiency of marine boilers...	240
	Reply to discussion on his Paper	251
Nicholson, John ...	*Method of arranging the Coal Bunkers of a Steamer so as to reduce the Ballast to a Minimum*	98
	Reply to discussion on his Paper	110
Noel, Captain G. H., R.N. ...	*A practical Measurement of the comparative fighting Efficiency of Ships of War*	1
	The distribution of armour in the *Admiral* type of ship ...	19
	Reply to discussion on his Paper	27
	Necessity of being able to compare fighting efficiency of different types of ships of war	28
	Distribution of armour in war ships	28
	Coal supply of war ships	29
	Torpedo defence of war ships	29
Paris, Admiral E. ...	Method of obtaining a mean cross section of a ship ...	71
Parker, W.	Relative performances of compound and triple compound engines in similar boats	148

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Name of Writer or Speaker.	*Title of Paper*, or Subject under Discussion.	Page.
Parker, W.	Boilers of the *S.S. Aberdeen*	149
	Injurious effect produced by cylinder oil in boilers	149
	Forced draught as applied to furnaces of merchant steamers	250
	Experience in the Use of thick Steel Boiler Plates	253
	Reply to discussion on his Paper	274
President	Alteration of Rules 39 and 40	xxiv
	Opening Address	xxiv
	Captain Noel's Paper on the comparative fighting Efficiency of Ships of War	29
	Mr. P. Watt's Paper on the use of Water Chambers for reducing the Rolling of Ships at Sea	49
	Mr. J. H. Heck's Paper on a mechanical Method of measuring a Vessel's Stability	72
	Mr. F. P. Purvis's Paper on Stowage of Steam Ships and Mr. J. Nicholson's Paper on a Method of arranging the Coal Bunkers of a Steamer	110
	Captain J. C. Tuxen's Paper on Yacht Measurement and Time Allowance for Racing	128
	Mr. J. I. Thornycroft's Paper on the most suitable Propeller for shallow Draught	140
	Mr. A. E. Seaton's Paper on some further Experience with Triple Compound Engines	153
	Mr. Mac Farlane Gray's Paper on the theoretical Duty of Heat in the Steam Engine	166
	Mr. M. H. Robinson's Paper on the Application of moderate forced Draught to Furnaces of small Steam Vessels ...	178
	Mr. J. G. Wildish and Mr. J. T. Milton's Papers on riveted Joints	226
	Mr. D. Purves's Paper on the Manufacture of large Forgings for Stern Frames	239
	Mr. J. T. Milton's Paper on the Efficiency of Marine Boilers ...	252
	Point of order	260
	Mr. W. Parker's Paper on Experience in the Use of thick Steel Boiler Plates	277
	Mr. E. A. Linnington's Paper on the propelling Machinery of high speed Ships	288
	Mr. D. Joy's Paper on the Joy's Valve Gear	297
	Replying to vote of thanks	303
	Vote of thanks to the Secretary	305
Purves, D.	*On the Manufacture of large Forgings for Stern Frames* ...	227
	Reply to discussion on his Paper	239
Purvis, F. P.	Mr. Heck's apparatus for calculating the stability of a ship ...	69
	The Stowage of Steamships	74
	Reply to discussion on his Paper	110
Putman, W.	Manufacture of forged stern frames	236
	Relative value of different kinds of welds	237
	Steel and iron railway axles compared	237
Quelch, R. J. .:. ...	Application of moderate forced draught to the *S.S. Brenda* ...	176

Digitized by Google

Name of Writer or Speaker.	*Title of Paper*, or Subject under Discussion.	Page.
Quelch, R. J.	Heating surface in marine boilers	176
Reed, Sir Edward J. ...	Distribution of armour in the *Admiral* type of ship	19
	Comparative efficiency of war ships	20
	Disadvantage of building large numbers of rams and torpedo boats	20
	Displacement as an approximate estimate of relative value of ships	20
	Criticisms on Captain Noel's Paper	21
	Criticisms on the *Admiral* type of ships	21
	Nominal coal carrying power of war ships	22
	Absence of armour protection to stability in *Admiral* type of ship	26
	Use of water chambers in checking the rolling of ships... ...	43
	Labours of Englishmen and Frenchmen in increasing our knowledge of science of stability	67
	Mr. Heck's apparatus for mechanically measuring a ship's stability	67
	Admiralty experiments on the strength of riveted joints ...	217
	Desirability of collating the results obtained by various experimenters on the strength of riveted joints	217
	Point of order	260
	Efficiency of twin screws as affected by depth of draught ...	287
	Twin screws for mercantile steamers	287
	Proposing vote of thanks to the Society of Arts	305
Reid, J.	Advantages of heavy lead keels in yachts	127
Rennie, G. B.	Early experiments on guide blade propellers	136
	Seconding vote of thanks to the Society of Arts	305
Rigg, A.	His own experiments on guide blade propellers	132
	Advantages of guide blade propellers	133
Robinson, M. H.	Guide blade propellers	138
	The Application of moderate forced Draught to the Furnaces of small Steam Vessels upon Mr. P. W. Willan's System	167
	Application of forced draught to small steam vessels by Messrs. Thornycroft & Co.	175
	Reply to discussion on his Paper	177
	Fouling of tubes in marine boilers	250
	Seconding vote of thanks to the Council	304
Robinson, Admiral Sir R. Spencer	Difficulty of comparing the relative efficiency of different types of ships	23
	Protection given to stability of the *Admiral* type of ship ...	23
Rodger, G.	Two-crank compared with three-crank compound engines ...	151
	Quadruple compound engines	151
Samuda, J. D'A. ...	Alteration of Rules 39 and 40	xxiv
	Captain Noel's Paper on the comparative fighting efficiency of ships of war	24
	Proper function of armour in war ships	24
Scott, —	Mr. Heck's method of calculating the stability of a ship ...	70
Seaton, A. E.	*Some further Experience with triple Compound Engines* ...	141
	Reply to discussion on his Paper	152
	Economy of triple compound engines	152
	Two-crank compared with three-crank compound engines ...	152

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Name of Writer or Speaker.	*Title of Paper*, or Subject under Discussion.	Page.
Seaton, A. E.	Accidents to forged stern frames	233
	Forging of stern frames	233
	Built up stern posts	234
	Improvements in the connection between stern frames and the structure of ships	234
	Relative value of different systems of welding	235
	Fuel economy as affected by the steam pressure	248
	Proportion of tube surface and grate surface in marine boilers	248
	Mr. McFarlane Gray's method of computing coal consumption in marine boilers	249
	Influence of size of combustion chamber on the durability of tubes	249
	Forced draught for marine boilers	249
	Failure of the steel plate referred to in Mr. Parker's Paper ...	270
	Twin screws in mercantile steamers	286
	Loose couplings far stern shafts	287
	Proposing vote of thanks to the Council	304
Selwyn, Admiral J. H. ...	Guide blade propellers	135
Seoane, Captain P. ...	*A Diagram of Stability*	298
Thompson, C.	Triple compound engines of the *S.S. Aberdeen*	150
Thornycroft, J. I. ...	*Most suitable Propeller for shallow Draught*	129
	Reply to discussion on his Paper	138
	Difference between Rigg's and Thornycroft's guide blade propellers	138
	Suitability of guide blade propellers for river steamers ...	139
	Weight of high speed engines	140
Tuxen, Captain J. C. ...	*On Yacht Measurement and Time Allowance for Racing* ...	111
Watts, P.	*The Use of Water Chambers for reducing the Rolling of Ships at Sea*	30
	Reply to discussion on his Paper	48
	Application of water chambers to checking the rolling of merchant ships	48
	Effect of variation of shape of water chambers	49
	Experiments with water chambers on *H.M.S. Inflexible* ...	49
West, H. H.	Mr. Heck's apparatus for calculating the stability of a ship...	70
	Measurement of the cargo capacity of ships	108
	Specific gravity of various descriptions of cargo	109, 110
	Application of moderate forced draught to small steamers ...	177
	Mr. Milton's Paper on the strength of riveted joints ...	217
	Use of steel rivets in joining steel plates	218
	Machine compared with hand riveting	218
	Strength of riveted joints to resist alternating strains	218
	Example of injury to riveted joints in the bottom of an iron ship	219
	Failure of the steel plate referred to in Mr. Parker's Paper ...	271
	Use of high tensile steel under the rules of the Underwriters' Registry	272
White, W. H. ...	Difficulty of comparing relative efficiency of different types of ships	25
	Protection given to stability of the *Admiral* type of ship ...	26, 27
	The *Italia* class of ship	26
	Origin of water chambers for checking the rolling of ships ...	44

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Name of Writer or Speaker.	*Title of Paper*, or Subject under Discussion.	Page.
White, W. H.	Effect of water chambers compared with bilge keels in checking the rolling of ships	45, 48
	Model experiments on rolling	45
	Mr. Heck's method of calculating the stability of a ship ...	70
	Specific gravity of various descriptions of cargo	107
	Arrangement of coal bunkers in war ships	108
	Captain Tuxen's career	124
	Early experiments on guide blade propellers	137
	Use of paddle wheels on river steamers	137
	Future of guide blade propellers	137
	Friction of riveted joints	219, 221
	Desirability of collating the results obtained by various experimenters on the strength of riveted joints	221
	Iron compared with steel rivets in the joints of steel plates ...	222
	Admiralty experiments on the strength of riveted joints ...	222
	Importance of countersinking the holes for steel rivets ...	223
	Machinery of high speed steamers	285
Wildish, J. G.	*On the Strength of mild Steel Plates and Rivets of various Kinds used in Shipbuilding*	179
	Explanation of diagrams illustrating his Paper on riveted joints ...	213, 214
	Reply to discussion on his Paper	223
Wright, J.	Economy of fuel with forced draught	247
	On Mr. Joy's valve gear	295
	Replying to vote of thanks to the Council	305
Yarrow, A. F.	Relative advantages of paddles and guide blade propellers for river steamers	134

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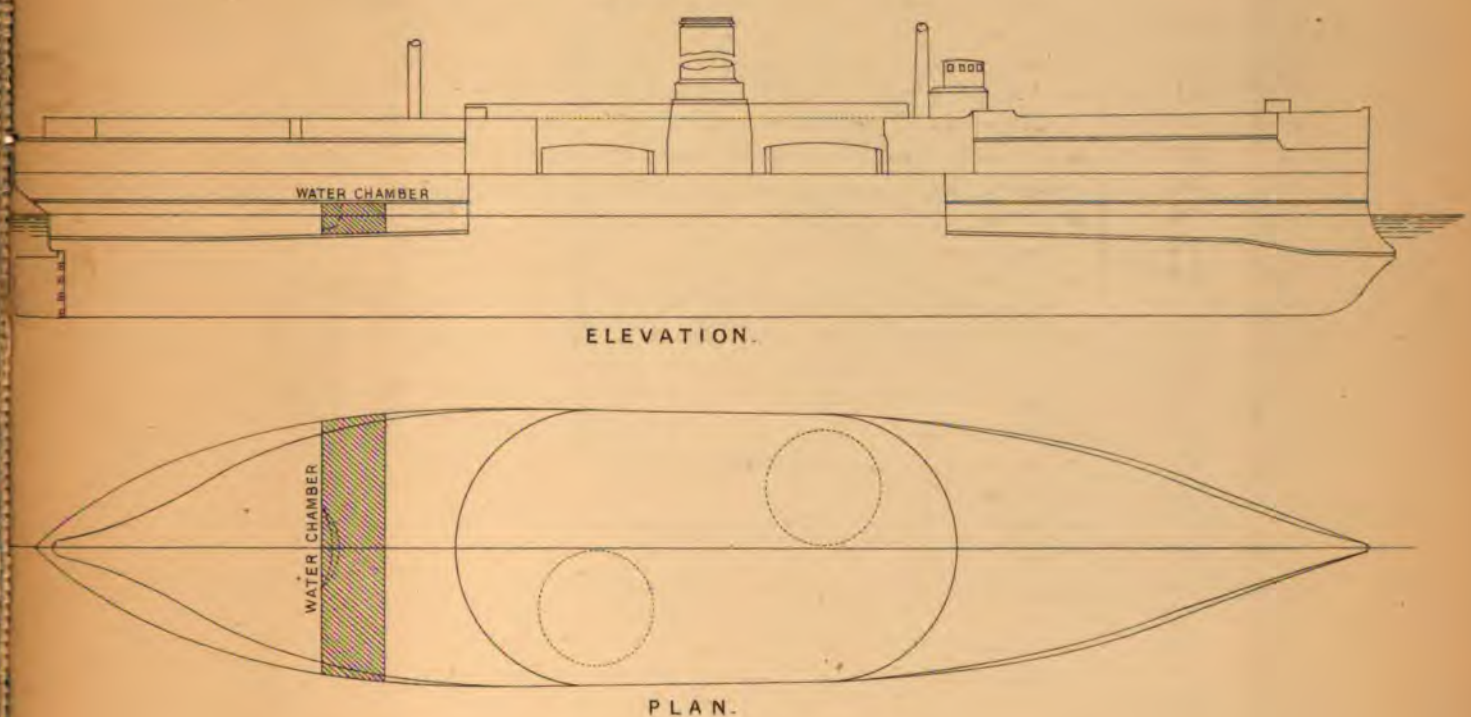
To Illustrate Mr. P. Watts' Paper on the Use of Water Chambers for reducing the Rolling of Ships.

FIG. 1.



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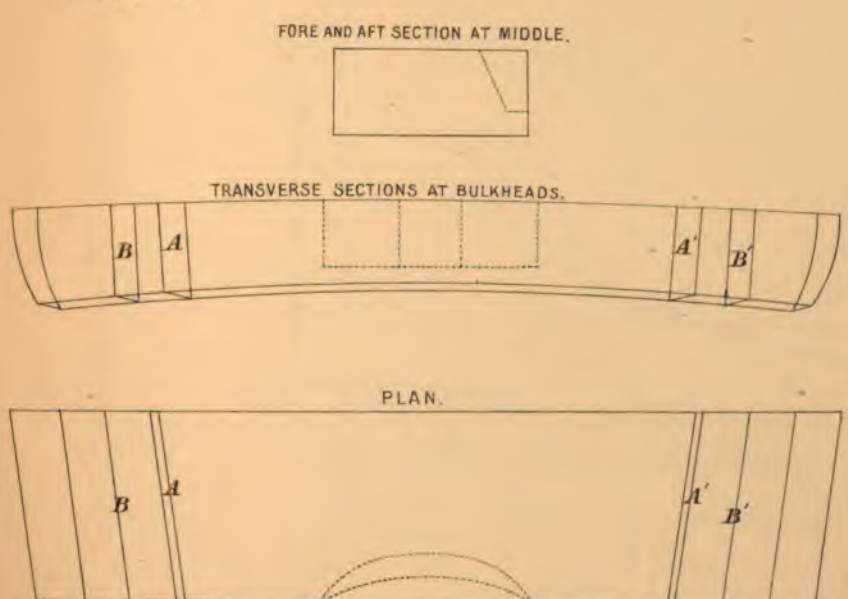

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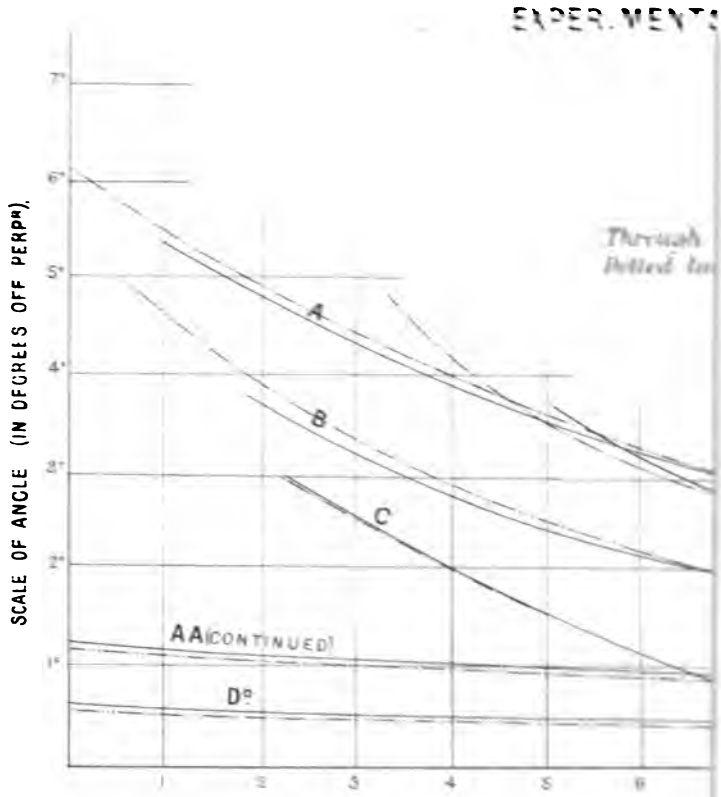

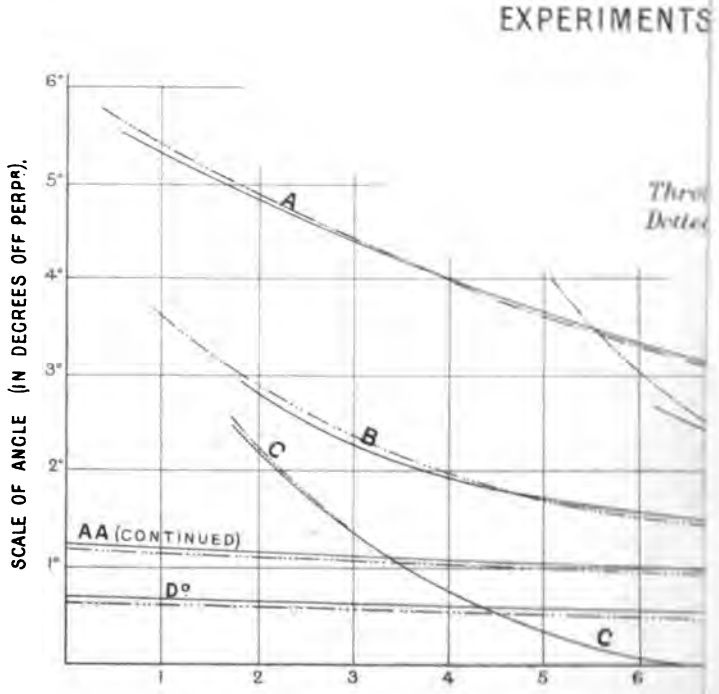

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FIG. 5.

EXPERIMENTS ON SHIP AND MODEL COMPARED. CURVES OF EXTINCTION LUE.

NARROW CHAMBER OF 43 FEET MEAN WIDTH.

Through Lines ..... *Ship.*
Dotted Lines ..... *Model.*

A A ..... *Chamber empty.*
B B ..... *19·7 tons of water in Ship, (5 lbs. in model).*
C C ..... *43·3 tons of water in Ship, (11 lbs. in model).*
D D ..... *78·8 tons of water in Ship, (20 lbs. in model).*

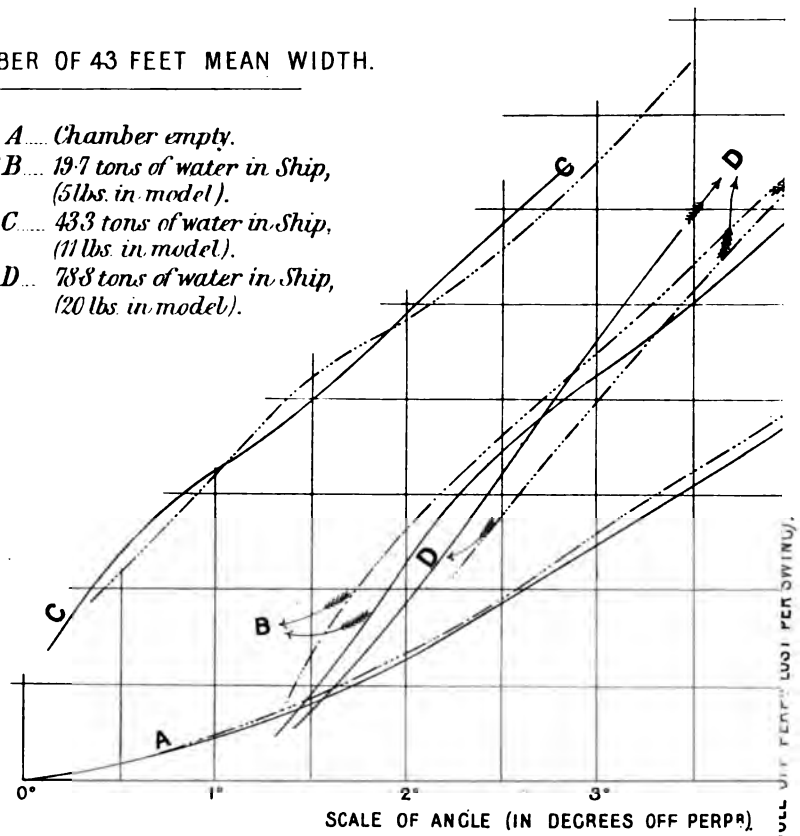

FIG. 7.

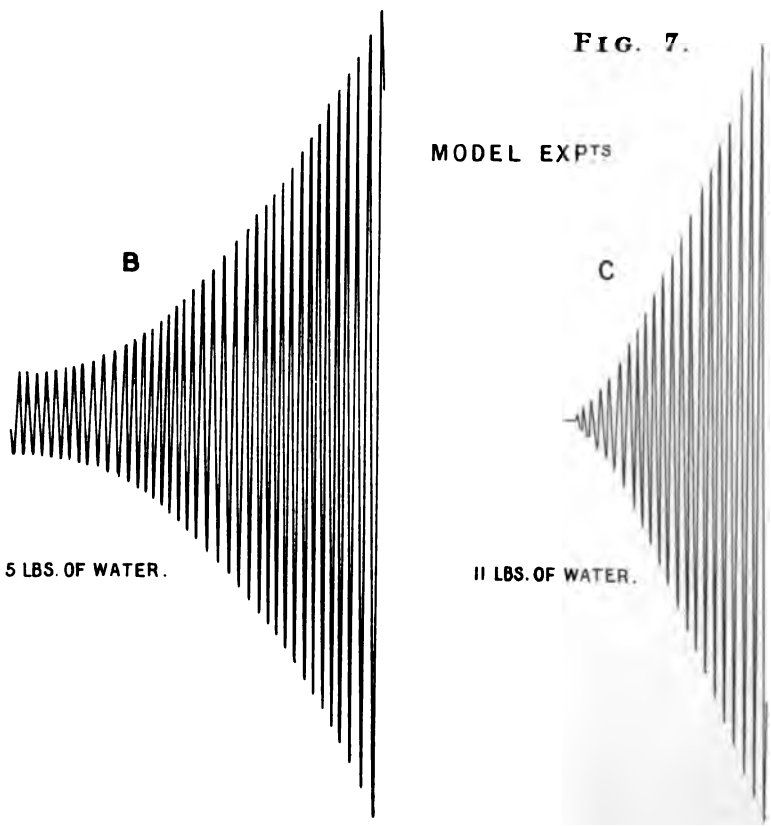

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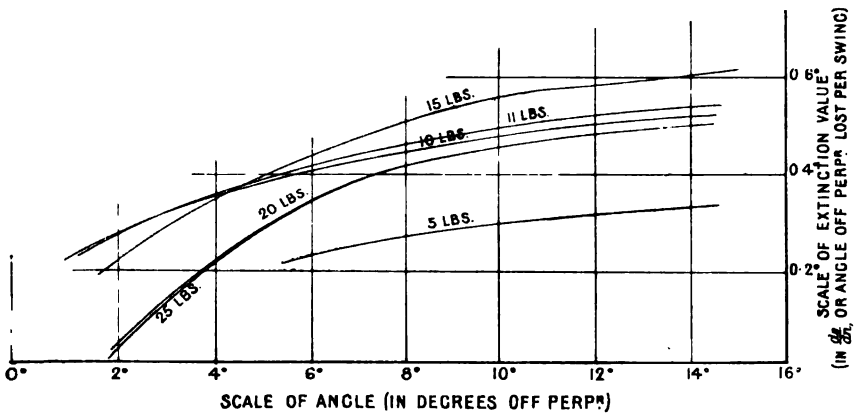
FIG. 8.

MODEL EXPERIMENTS.

CURVES OF EXTINCTION VALUE OF WATER CHAMBER ONLY.

NARROW CHAMBER, CORRESPONDING TO 43 FEET MEAN WIDTH IN SHIP
ASSUMED STABILITY 872 FOOT LBS. (68,750 FOOT TONS IN SHIP.)



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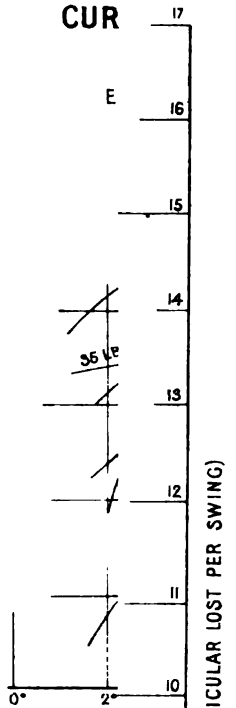

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SHOWING THE

RESISTANCES DUE TO WATER

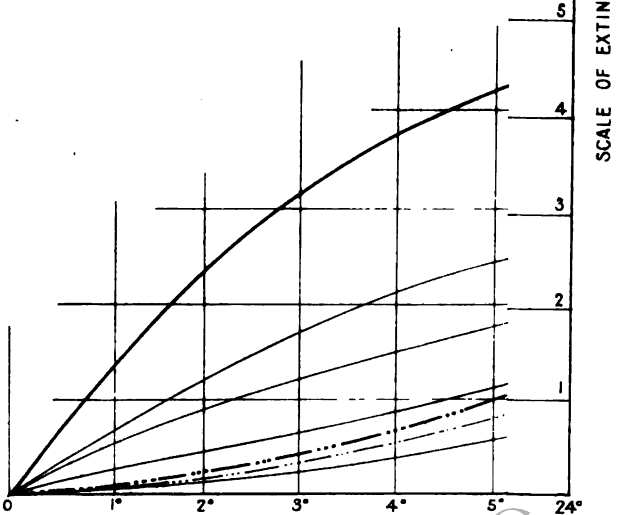

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FIG. 12.

STEADY ROLLING IN COPERIODIC WAVES.

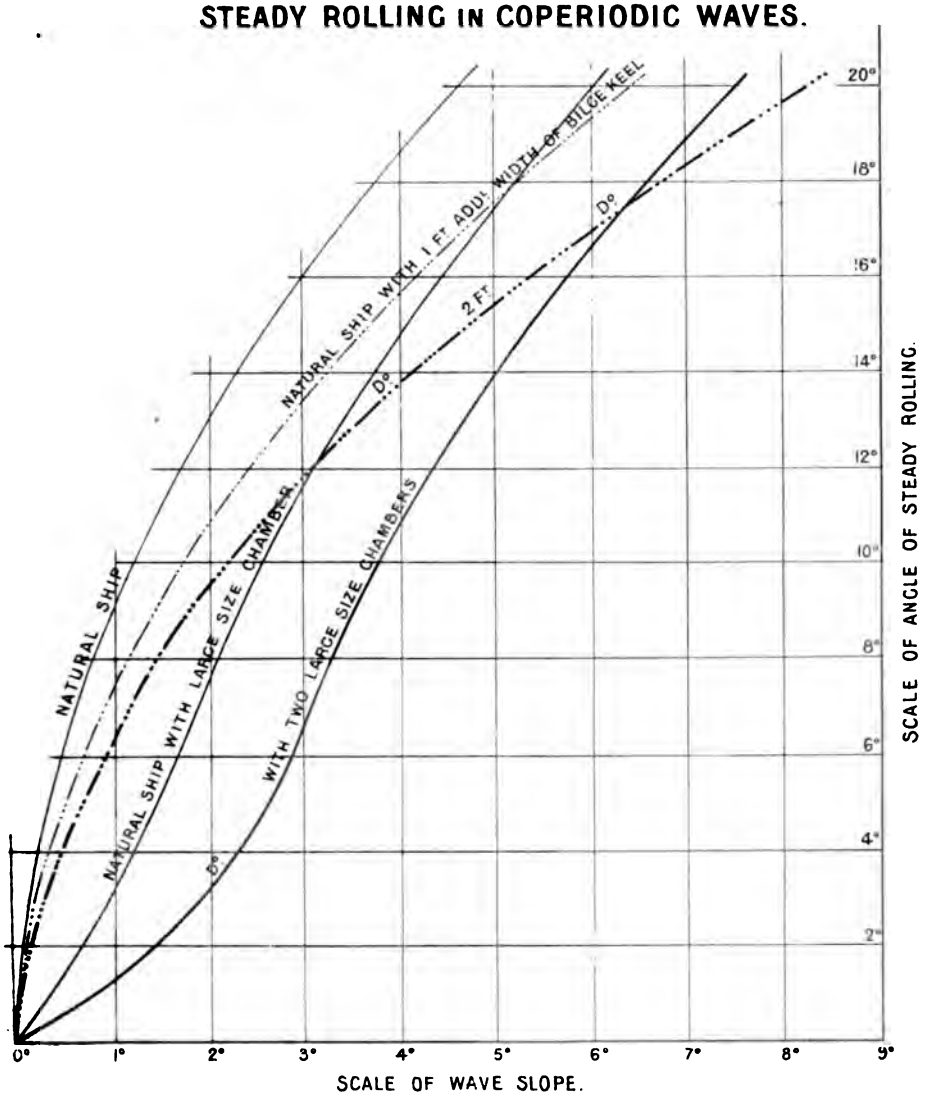
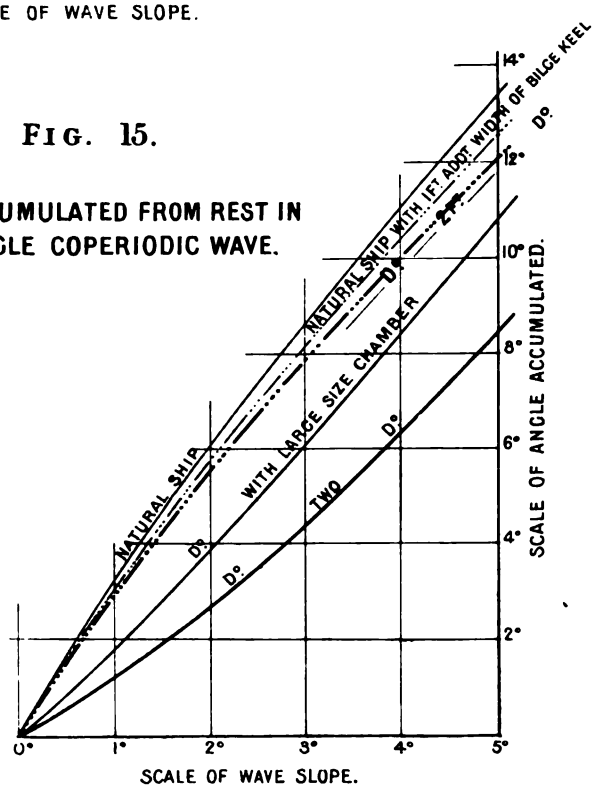


FIG. 15.

ANGLE ACCUMULATED FROM REST IN ONE SINGLE COPERIODIC WAVE.



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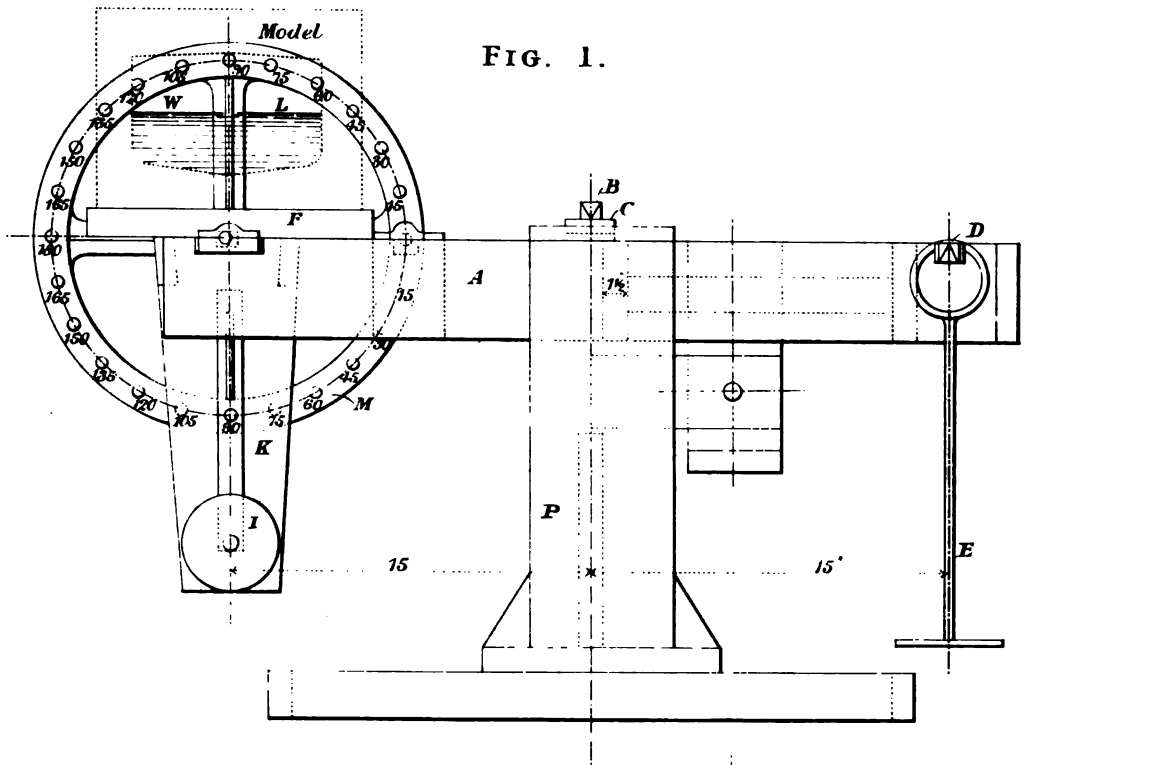
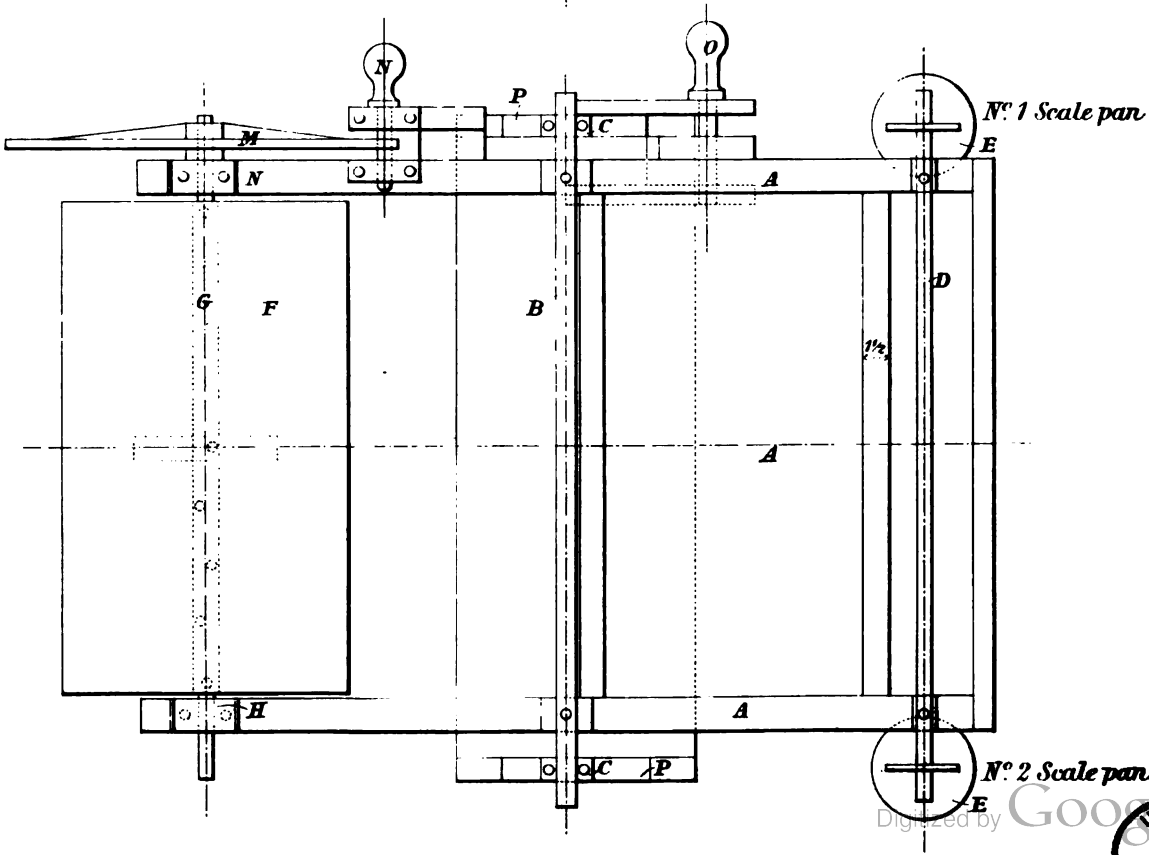
To Illustrate Mr. J. H. Heck's Paper on a mechanical Method of measuring a Vessel's Stability.

STABILITY BALANCE FOR INSIDE SECTIONAL MODELS.

Scale 1½ inches 1 foot





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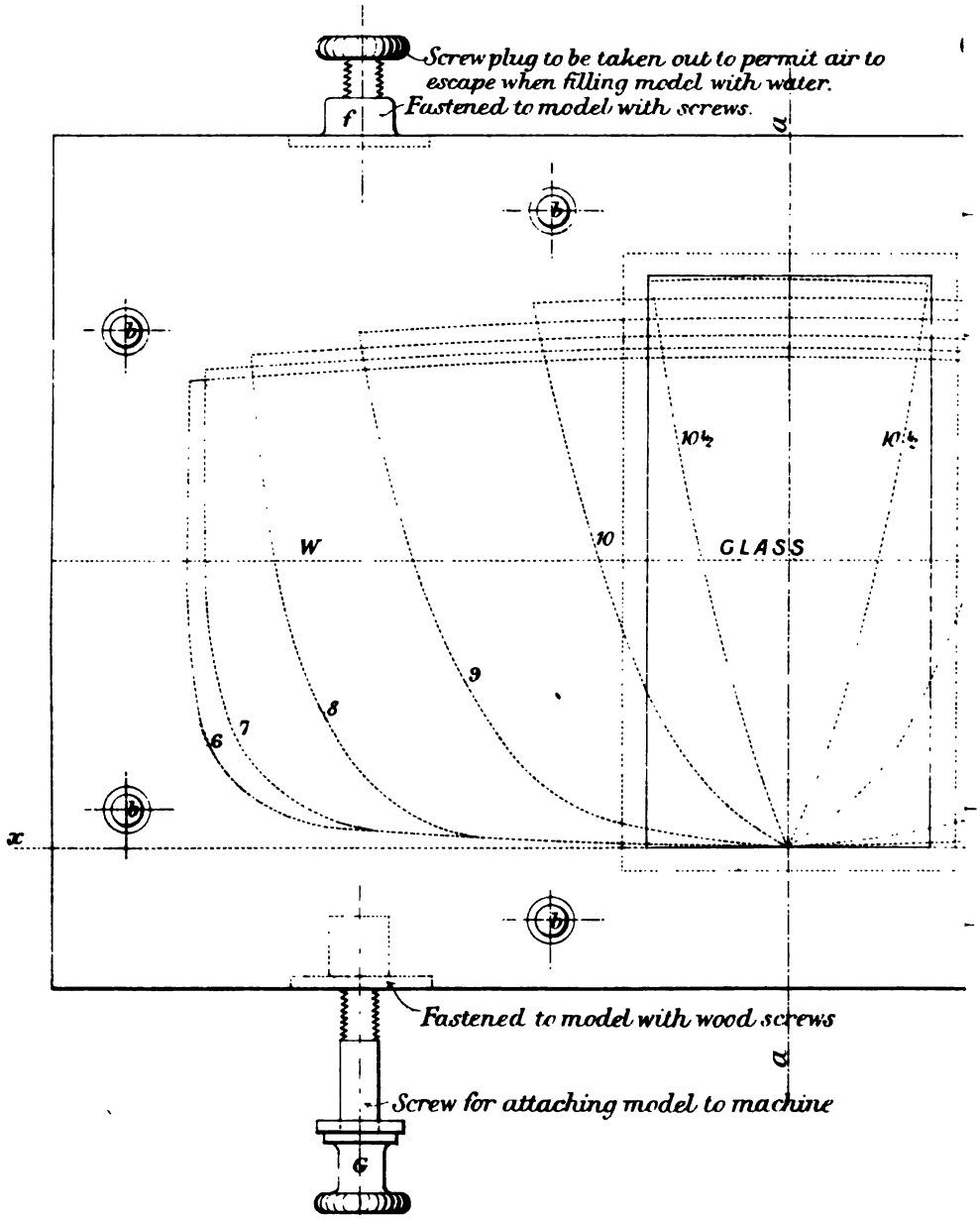
To Illustrate Mr.



Internal sections from No. 1 to No. 5 not shown on body plan model so as to avoid confusion.
The brass fittings for filling model with water letting air e when filling with water and for attaching model to mac can be taken off and used for other models.

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To Illustrate Mr. J. H.

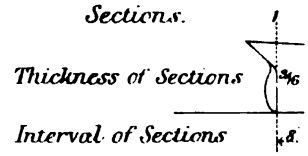
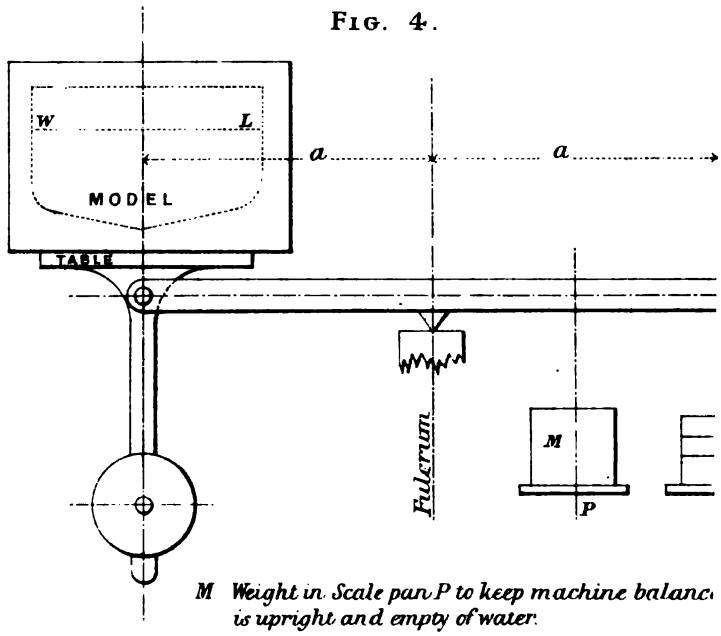


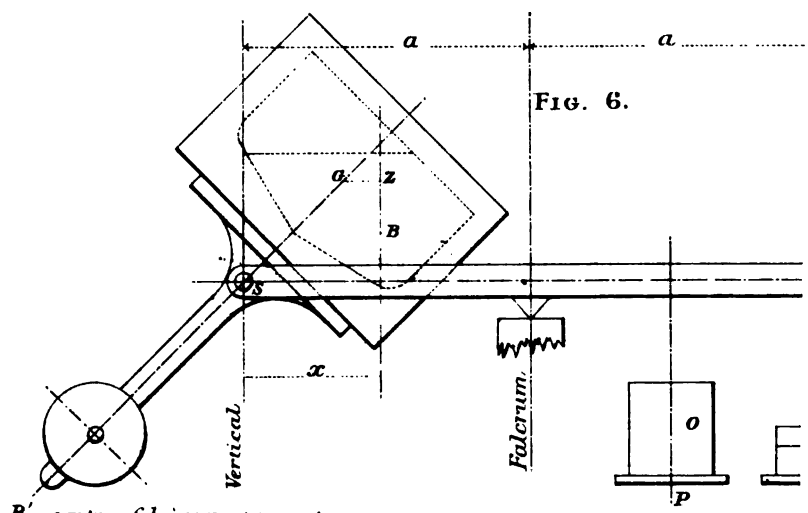
FIG. 4.



M Weight in Scale pan P to keep machine balanc
is upright and empty of water.

D Known weight in Scale pan E to keep machine b
model is upright and filled with water up to W

D is evidently to the weight of water in model.



B′ centre of buoyancy or centre of gravity of water in model.

O Weight in Scale pan P to keep machine balanced when mod
at 45° to left of Vertical and empty of water.

W′ Total known weights in Scale pan E to keep machine balan
model is filled with water to water W L and inclined at 4
vertical.

x Horizontal distance between centre of buoyancy and axis o

D as before weight of water in model.

Evidently $Wa = D(a-x)$

$$\therefore x = a - \frac{Wa}{D}$$

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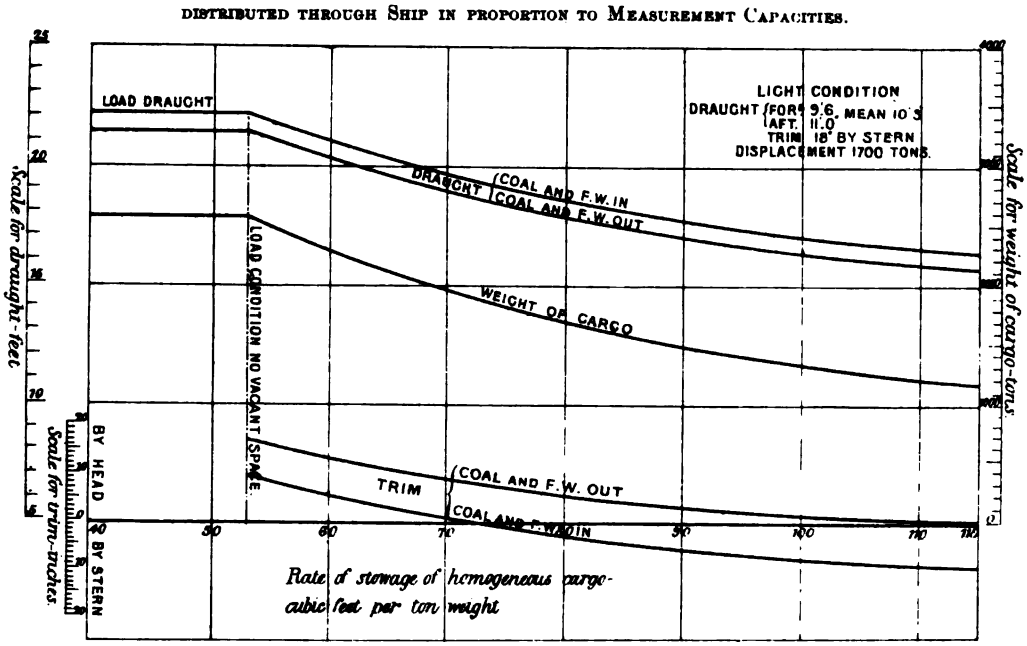
To Illustrate Mr. J

THREE DECK STEAMER.

FIG. 2.

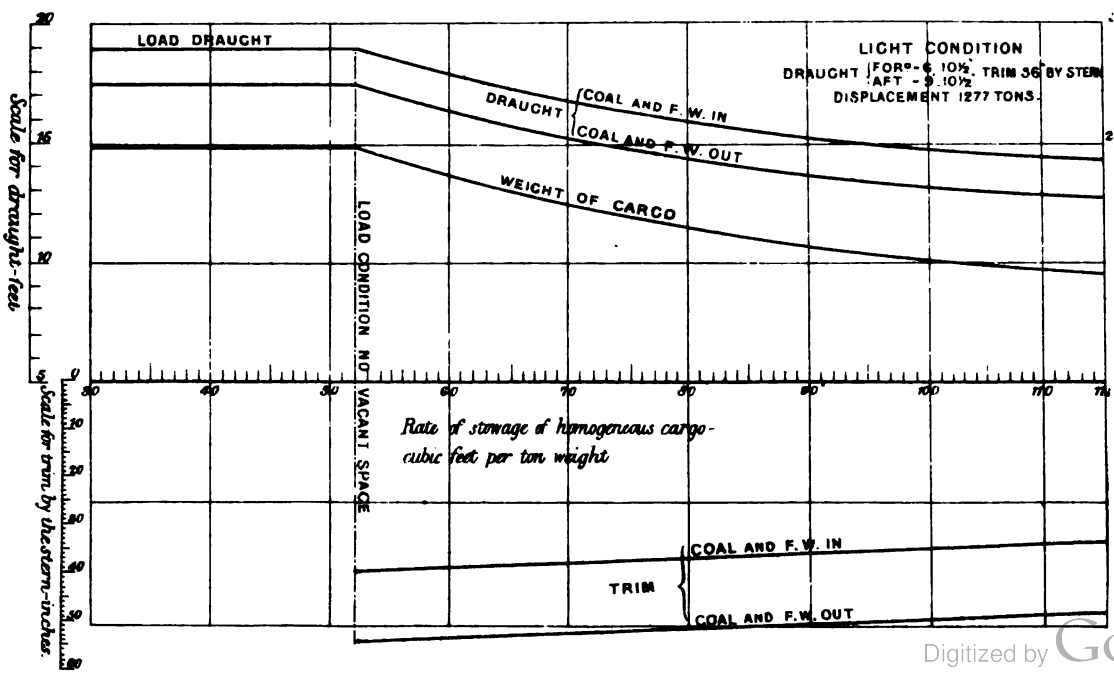
CURVES TO SHOW TRIM OF VESSEL WHEN LOADED WITH HOMOGENEOUS CARGO DISTRIBUTED THROUGH SHIP IN PROPORTION TO MEASUREMENT CAPACITIES.



QUARTER DECK STEAMER.

FIG. 3.

CURVES TO SHOW TRIM OF VESSEL WHEN LOADED WITH HOMOGENEOUS CARGO DISTRIBUTED THROUGH SHIP IN PROPORTION TO HER MEASUREMENT CAPACITIES.



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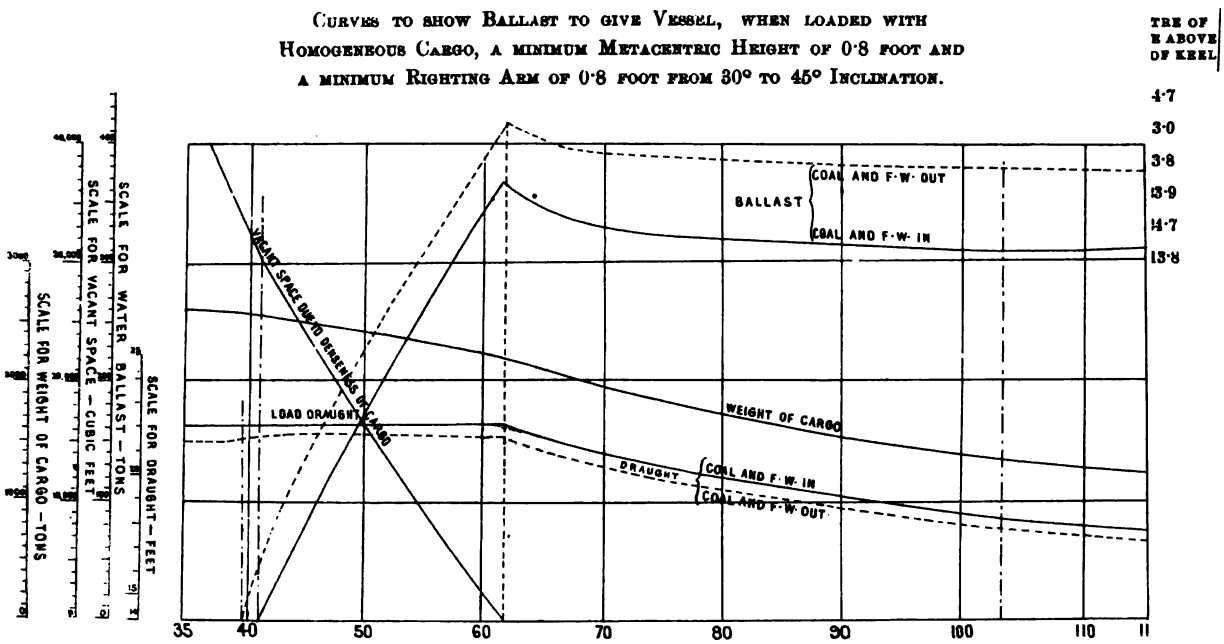
To Ill:

THREE-DECK STEAMER.

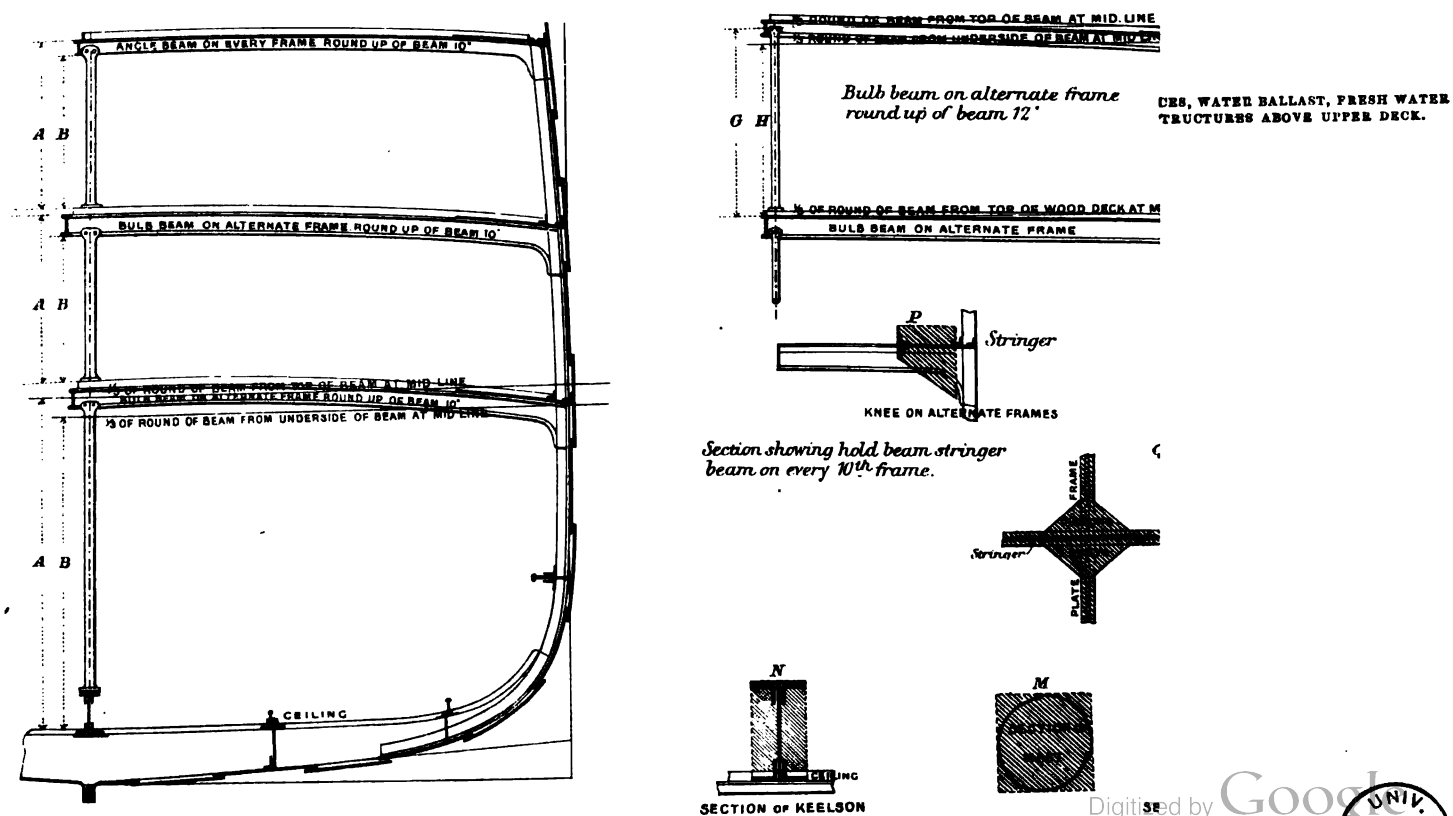
Fig. 4.

Curves to show Ballast to give Vessel, when loaded with Homogeneous Cargo, a minimum Metacentric Height of 0·8 foot and a minimum Righting Arm of 0·8 foot from 30° to 45° Inclination.



Rate of Stowage of homogeneous cargo Cubic feet per ton weight.

Fig. 8.



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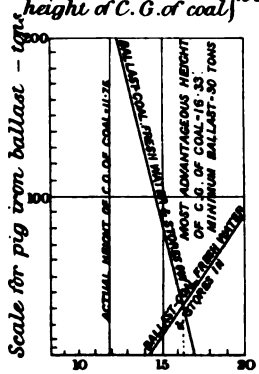
To Illustrate Mr. J. Nicholson's Paper o

Fig. 1.

STEAMER WITH SINGLE BOTTOM.

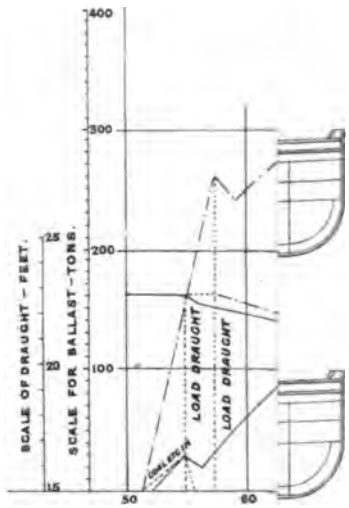
ST

Load draught 25.3'
" displacement 8940 tons
Safe height 18·65 ft.
Weight of coal 797 tons
Most advantageous height of C.G. of coal 16·33 ft



Scale for Height of C.G. of coal – feet.

(8036)

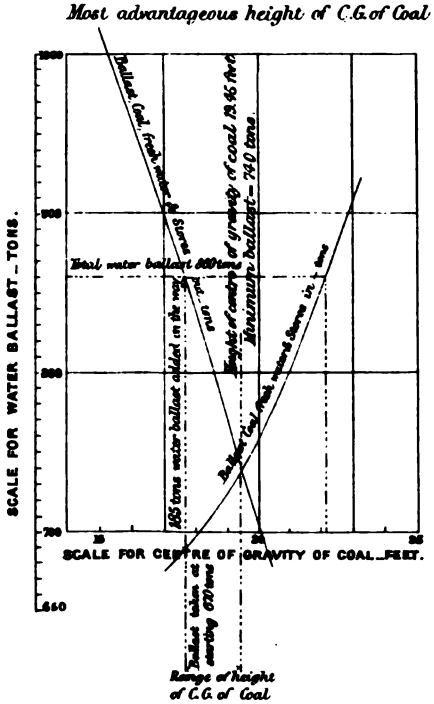


(8036)

Fig. 4.

STEAMER WITH DOUBLE BOTTOM.

Load draught 26'. 1"
Load displacement 8640 tons
Weight of Coal 2038 tons
Most advantageous height of C.G. of Coal 13.45 ft.



9'
5'

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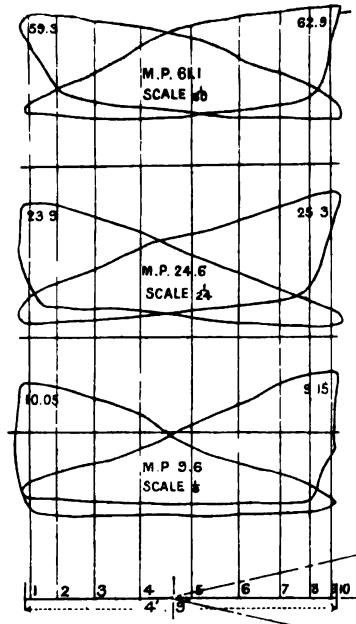
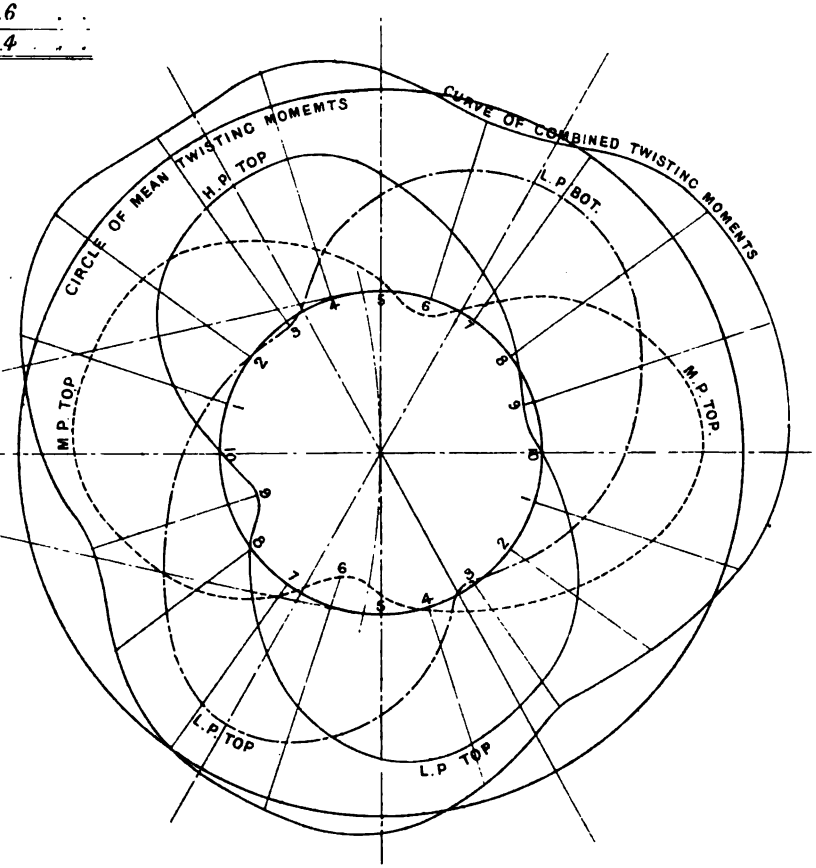
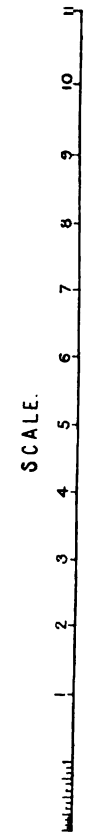


REVOLUTIONS 56.5

H.P.	750.1	I.H.P
M.P.	785.7	. . .
L.P.	824.6	. .
Total	2360.4	. . .

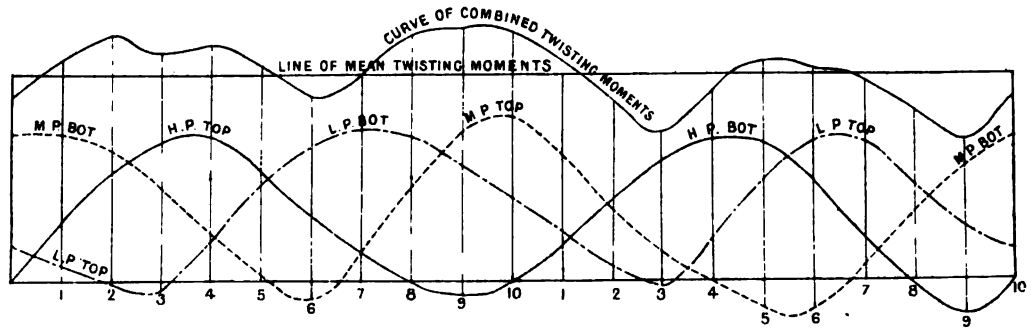




ENGINE.

This Crank Triple-Compound.
H.P. Cyl 31" dia
M.P. . 50" .
L.P. . 80" .
Stroke 57"
Cranks at 120° H.P. leading
Boiler Press 150 lbs.
Centres of Connecting-rod 11.0"

Maximum / Mean = 1·23
Scale 1 inch = 192000 ft lbs.



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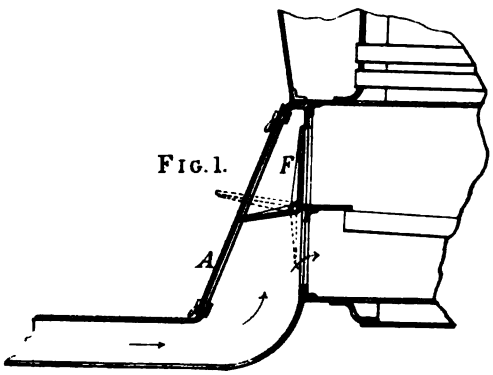

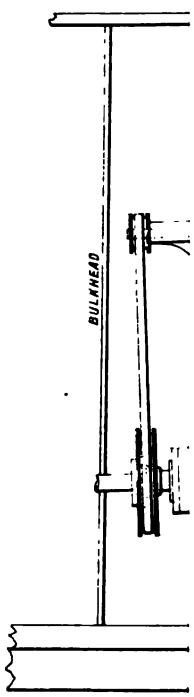

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To Illustrate Mr. J. T. Milton's Paper on some Notes on the Strength of Riveted Joints.

FIG. 4.

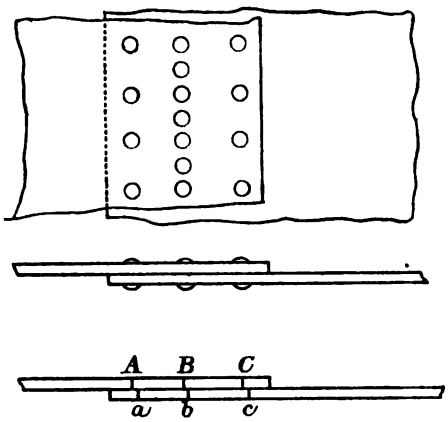


FIG. 3.

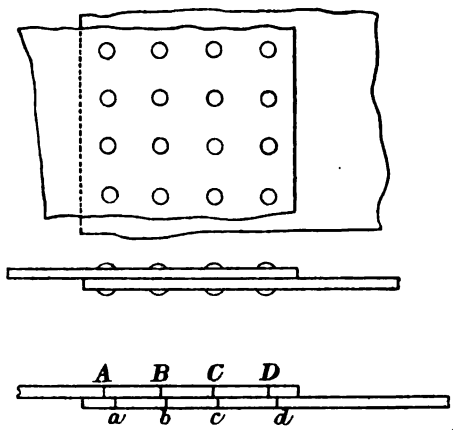


FIG. 1.

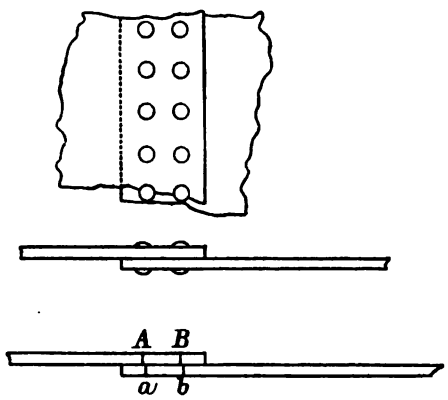


FIG. 2.

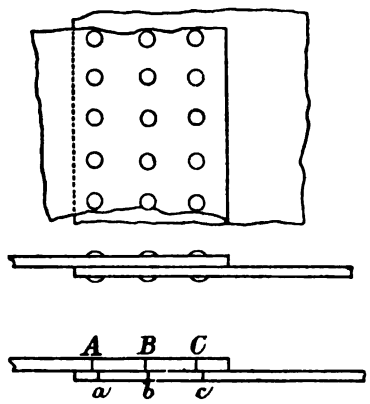


FIG. 6.

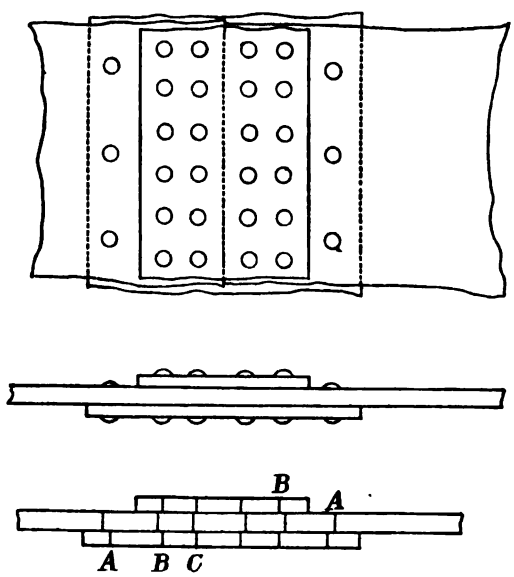
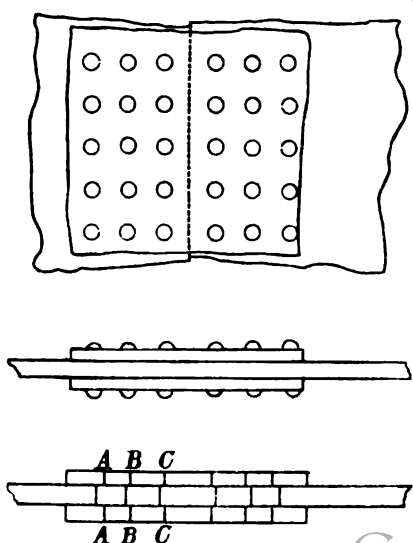


FIG. 5.



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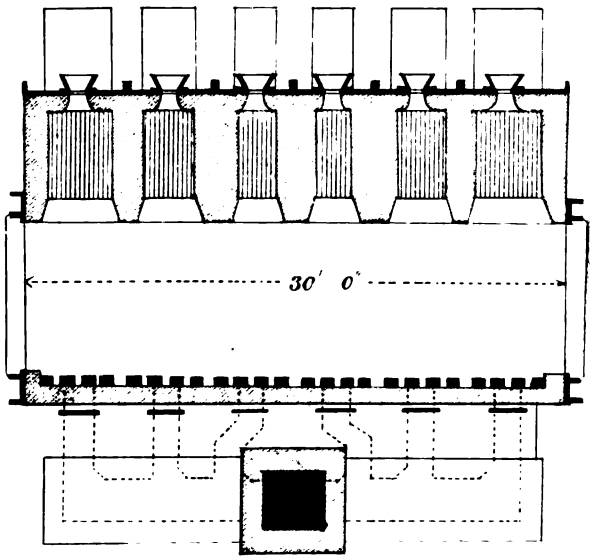

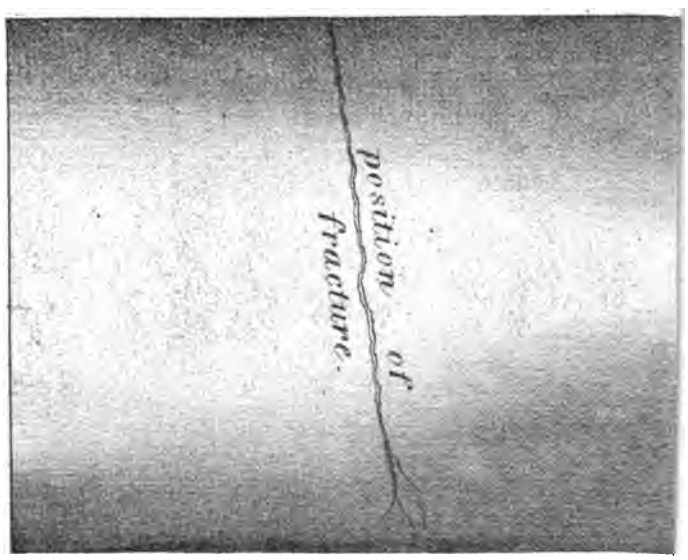

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To Illustrate Mr. David Joy's Paper on the Joy Gear i
and its Sp

Fig. 1.

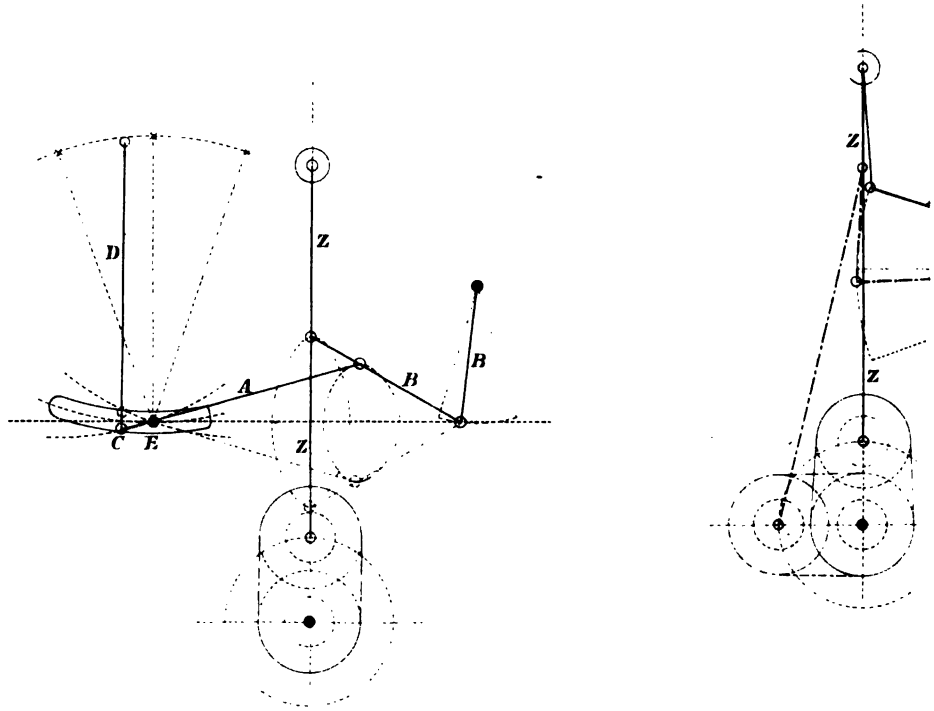
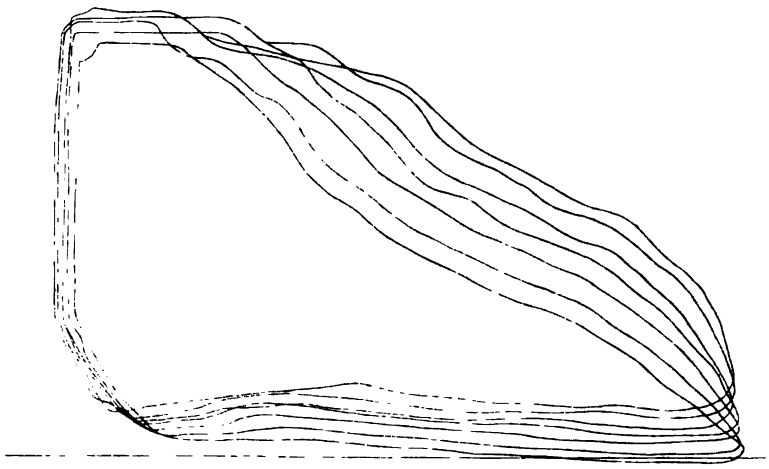


Fig. 2.



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